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THEORIES OF THE ORIGIN OF MOUNTAIN RANGES.

MOUNTAINS are the focal points of geological interest. In their complex structure are contained all kinds of rocks, sedimentary, eruptive and metamorphic; and in their formation are engaged all geological forces in their greatest intensity. They are the culminating points, the theatres of greatest activity of all geological agencies; igneous agencies in their formation, aqueous agencies by sedimentation in their preparation, and by erosion in their subsequent sculpture. Their discussion, therefore, is a summation of all the principles of structural and dynamical geology. But they are equally important in historical geology, for the birth of mountains marks the times of great revolutions in the history of the earth, and therefore determine the primary divisions of geological time. Evidently therefore the theory of mountains lies at the very basis of theoretic geology, and a true theory must throw abundant light on many of the most difficult problems of our science.

But if this is the most important, it is also the most difficult of all geological questions. My object now is to give, as briefly as possible, the present condition of science on this subject. But in all complex subjects there is a region of comparative certainty and a region of uncertainty; a region of light and a region of twilight. My farther object, therefore, will be to separate sharply these two regions from one another, and thus to clear the ground, narrow the field of discussion and direct the course of profitable investigation.

But first of all I must define my subject. A mountain range is a single *mountain individual*—born at one time (monogenetic) *i.e.*, the result of one—though it may be a prolonged—earth-effort; as contra-distinguished on the one hand from a mountain *system* which is a family of mountain ranges born at different times (polygenetic) in the same general region; and on the other from ridges and peaks which are subordinate parts—limbs and organs—of such a mountain individual. Now a theory of mountains is essentially a theory of mountain ranges, as thus defined. In all that follows, therefore, on the subject of mountain structure and origin, we refer to mountain ranges.

STRUCTURE OF MOUNTAINS.

The origin of mountains is revealed in their structure. We must, therefore, give briefly those fundamental points of structure on which every true theory of origin must be founded.

1. *Thickness of Mountain Sediments.*—The enormous thickness of mountain strata is well known, but it is impossible to overstate its fundamental importance. We therefore give some striking examples. The Palæozoic rocks involved in the folded structure of the Appalachian, according to Hall, are about 40,000 feet thick. The Palæozoics and the Mesozoics in the Wasatch, according to King, are about 50,000 feet thick. The Cretaceous alone, in the Coast Range of California near the Bay of San Francisco, according to Whitney, are 20,000, and in Shasta county, according to Diller, are 30,000 feet thick. The Mesozoics and Tertiaries of the Alps, according to Alpine geologists, are 50,000 feet.[1] The upper Palæozoic and Mesozoic of the Uinta, according to Powell, are 30,000 feet. These are conspicuous examples, but the same is true of all mountains.

It might be objected that these numbers express the general thickness of the stratified crust everywhere—only that in mountains the strata are turned up and their thickness exposed by erosion. But this is not true. For in many cases the strata may be traced away from the mountain; and in such cases they always *thin out* as distance increases. For example, the 40,000 feet of

[1] JUDD: Volcanoes, p. 295.

Appalachian Palæozoics thin out going west until at the Mississippi river they are only 2000 to 4000 feet. The Palæozoics which in the Wasatch are 30,000 feet thin out eastward until they are only 1000 feet on the plains. It follows then that *mountains are lines of exceptionally thick sediments.*

2. *Coarseness of Mountain Sediments.*—Mountains are composed mainly of grits, sandstones, and shales, *i.e.*, of mechanical sediments, and most conspicuously so along their axial regions. As we go from this region, sometimes in either direction, but especially in one direction, the strata become finer and finer; sandstones giving way to shales and shales to limestones, *i.e.*, mechanical to organic sediments. This is conspicuously true of the Appalachian; in so many ways a typical mountain. As we pass from the eastern ridge westward, grits and sandstones are replaced by shales and these by limestones. Therefore mountains are also *lines of exceptionally coarse sediments.*

3. *Folded Structure of Mountains.*—The folded structure of mountains is perhaps the most universal, and certainly the most significant, of all their features. But there is great variety in the degree and complexity of the foldings. Sometimes the mountain rises as *one* great fold. The Uinta is an example of this. Sometimes and oftener there are *several open* folds, like waves of the sea. The Jura is a good example of this. Sometimes and oftenest of all, there are *many closely appressed* folds. This is the case in the Coast Range of California, in the Appalachian, in the Alps, and probably in the Sierra. The Appalachian may be taken again as the type. In this range the folds are most numerous and most closely appressed in the axial region, and open out and die away in gentle waves as we go westward. Finally, sometimes in extreme cases, as in the Alps, the Pyrenees and probably the Sierra, the strata of the lateral slopes are thrust in under the central and higher parts, so that the strata of these central parts are overfolded outwards on one or both sides. This is the *Fan-structure*, so marked in the Alps and Pyrenees, where the underthrust and overfold are on both sides, but found also in the Appalachian and Sierra, where they are on one side only.

Amount of Folding.—Folded structure implies, of course, an alternation of anticlines and synclines. The number of these varies with the intensity of the folding. In the Coast Range there are apparently four or five anticlines and corresponding synclines. In the Sierra they cannot be counted, but there must be very many so closely appressed that the strata seem to be a continuous series dipping all in the same direction, *i.e.*, steeply toward the axis, for at least 30 miles. They cannot form a single series, for this would make an incredible thickness. It must be a series repeated several times by extreme folding; how many, it is impossible now to say. In the Appalachian, according to Claypole[1], there are about 19 anticlines and synclines in 65 miles and in one part—Cumberland valley—there are eight in 16 miles. In the Vaudoise Alps, according to Renevier, there are at least seven[2], and in Savoy as many as 15[3]. In many cases the foldings are so extreme that the strata first rise as folds, then are pushed over beyond the base as overfolds, and finally broken at the crest and upper limb of the fold is pushed over the lower limb many miles horizontally. In the Highlands of Scotland, according to Peach[4], by overthrust, the Archæan is brought over the Silurian and overrides it for ten miles. In the Rocky Mountains of Canada, according to McConnell[5], the Cambrian is brought over the Cretaceous and overrides it for seven miles. In the Appalachian of Georgia, according to Hayes[6], by overthrust, the Cambrian is made to override the Carboniferous for eleven miles.

4. *Cleavage Structure.*—Closely connected with the last, and having a similar significance, viz., lateral squeezing and mashing, is another structure—*cleavage*. This structure is often asso-

[1] Am'n Nat'st, Vol. 19, p. 257 and seq.

[2] Archives des Science, Vol. 59, p. 5, 1877.

[3] Archives, Vol. 28, p. 608, 1892, and 25, p. 271, 1893.

[4] Nature, Vol. 31, p. 29, 1884.

[5] Geol. Surv. Can. 1886, Rep. D. p. 33.

[6] Bull. Geol. Soc. Am. Vol. 2, p. 141.

ciated with folding and both with mountain ranges. It is not so universal as folding only because all kinds of strata are not equally affected by it; being well exhibited only in fine shales. It is important to observe that in slaty cleavage the *strike* of the cleavage planes is the same as that of the strata, and both the same as the trend of the mountain; and that the *dip* of the cleavage planes is nearly or quite vertical. Whole mountains are thus cleavable from top to bottom.

5. *Granite or Metamorphic Axis.*—Some mountains are made up wholly of folded strata. This is the case with the Appalachian, the Coast Range, and the Jura. But most great mountains consist of a granitic or metamorphic axis with stratified flanks. This is conspicuously the case with the Sierra, the Alps, and most other great mountains. So general is this, that the typical structure of ranges may be said to be—a granitic axis forming the crest, and stratified rocks, more or less folded, outcropping on the slopes. This very characteristic structure ought to be explained by a true theory of origin.

6. *Asymmetric Form.*—Mountains are not usually symmetric, with crest in the middle and slopes equal on the two sides. On the contrary they usually have a long slope on one side and a steeper, often a very abrupt, slope on the other. The crest or axis is not in the middle but nearer to one side. The earth-wave seems ready to break and often does break with a great fault on the steeper side. The Uinta is perhaps the simplest example. This range rises as a single great fold, but steeper on the north side where there is a fracture and fault of 20,000 feet vertical. Of course in this as in all cases the original fault-cliff has crumbled down to a steep slope, or even been destroyed entirely. The Sierra and Wasatch are remarkable examples of asymmetry. The Sierra rises on the west side from the San Joaquin plains near sea level by a gentle slope fifty to sixty miles long, reaches its crest near 15,000 feet high and then plunges down by a slope so steep, that the desert plains on the east, 4,000 to 5,000 feet above sea level is reached in six to ten miles. There is on this side a fault-cliff nearly 11,000 feet high. The Wasatch

has a similar form, except that the fault-cliff looks westward instead of eastward. It is true that the extreme asymmetry of these two mountains was given them long after their origin and by a different process to be presently described. But even before this last movement they were probably asymmetric, though in a less degree. The Appalachian is perhaps here again a typical mountain. Its long slope is to the west and its crest close to the eastern limit. The Alps, the Appenines, the Carpathians, and the Caucasus, according to Suess, are foreign examples of the same form.

There are many other interesting points of structure that might be mentioned, but they are less significant of mode of origin and therefore omitted in this rapid sketch.

ANOTHER TYPE OF MOUNTAINS.

I have given the main characteristics of mountains of the usual type, of which the Appalachian, the Coast Range, the Alps and Pyrenees may be taken as good examples. But there is another type, different in structure and in mode of origin, to which attention, I believe, was first called by Gilbert. It is doubtful if they are found anywhere except in the Basin and Plateau regions, and therefore the type may be called the Basin region type. The Basin and Plateau regions are broken by north and south fissures into great crust-blocks which by gravitative readjustment have been tilted, *i. e.*, one side heaved up and the other side dropped down, so as to form a series of north and south ridges and valleys. Each ridge rises by a long slope on one side to a crest and then drops by a steep fault-cliff on the other. The ridges therefore are extremely asymmetric but the asymmetry is produced in a different way from that of the usual type. In a word, these mountains seem to be the result of a series of enormous parallel faults. Such faults are common everywhere, but do not usually give rise to any inequalities which may be dignified by the term mountain: or if so at one time, have since been levelled by erosion. But those in the Basin region are on so grand a scale and so recent in time, that they form

very conspicuous orographic features. I have sometimes doubted whether they should be called ranges at all; but when we reflect that at least 10,000 feet of the height of the Sierra is due to normal faulting, it seems impossible to withhold the term. Thus mountains may be divided into two types, viz., mountains formed by folding of strata and mountains formed by tilting of crust-blocks. The structure of the one is *anti*clinal or *di*clinal, of the other *mono*clinal. The Sierra probably belongs to both types. It was formed at the end of the Jurassic as a mountain of the first type, but the whole Sierra block was tilted up on its eastern side without folding, at the end of the Tertiary, and it then became also a mountain of the second type.

A complete theory must explain this type also; but since from its exceptional character it must be regarded as of subordinate importance, we shall be compelled to confine our discussion to mountains of the usual type.

EXPLANATION OF THE PRECEDING PHENOMENA.

In all cases of complex phenomena there have been many theories, becoming successively more and more comprehensive. The citadel of truth is not usually taken at once by storm, but only by very gradual approaches. First comes the collection of carefully observed facts. But bare facts are not science. They are only the raw material of science. Next comes the grouping of these facts by laws more or less general. This is the beginning of true science. Every such grouping or reducing to law is a scientific explanation, and therefore in some sense a theory. At first the grouping includes only a few facts. The explanation or theory lies so close to the facts as to be scarcely distinguishable from them. It is a mere corollary or necessary inference. It is modest, narrow, but also in the same proportion *certain*. Then the group of explained facts becomes wider and wider, the laws more and more general, and the theory more daring (but in the same proportion also perhaps more doubtful): until it may at last include the Cosmos itself in its boundless but shadowy embrace.

Now in this gradual approach toward perfect knowledge, there

are two very distinct stages. The one consists of explanation of the immediate phenomena in hand. This gives the laws of the phenomena, and may be called the *Formal Theory*. The other explains the cause of these laws, and may be called the *Causal or Physical Theory*. All science passes through these two stages. For example: Until Kepler, the phenomena of Planetary motion were a mere chaotic mass of observed facts without uniting law. Kepler reduced this chaos to order by the discovery of the three great laws which go by his name. This is the *formal* theory of Planetary motion. But still there remained the question, why do planets move according to these beautiful laws? Newton explained this by the law of gravitation. This is the causal or *physical* theory.

But this is so important a distinction that I must illustrate by examples taken from geological sclence. All the phenomena of slaty cleavage are completely explained by supposing the whole rocky mass to have been mashed together horizontally and extended vertically. This is the Formal theory and may be regarded as certain. But still the question remains: How does mashing produce easy splitting in certain directions? The solution of this question is the Physical theory, and is perhaps a little more doubtful, though I think satisfactorily answered by Tyndall. But still there remains a deeper and more doubtful question, Whence is derived the mashing force? Is it general interior contraction, as some think, or is it local expansion as others think. A perfect theory must answer all these questions. Take another example: All the phenomena of the drift are well explained by the former existence of an ice sheet moving southward by laws of glacial motion, scoring, polishing, and depositing in its course. This is the Formal theory. But still the question remains, What was the cause of the ice sheet? Was it due to northern elevation, or to Aphelian winter concurring with great eccentricity of the earth's orbit? And if due to northern elevation, what was the cause of that elevation? A perfect theory must answer all these questions. Take one more example: All the phenomena of earthquakes are completely explained by the emergence on the surface

and a spreading there from a centre, of a series of elastic earth-waves. This is the Formal theory. It explains the immediate facts observed here on the surface, but no more. But still remains the question, What is the *cause*, deep down below, of the concussion which determined the series of earth-waves. This, the physical theory, is far more doubtful. Or the theory may be made still deeper and proportionately more doubtful. If our theory of the cause of the interior concussion be the formation of a fissure or readjustment of a fault, as seems in many cases probable, there would still remain the question of the cause of great fissures and of their subsequent readjustment by slipping. This is probably as far as geological theory would go: for although cosmogony may go still farther, the interior heat of the earth is usually the final term of strictly geological theories.

I have made this long detour because I wish to keep clear in the mind these two stages of theorizing in the case of Mountain Origin. The formal theory is already well advanced toward a satisfactory condition; the physical theory is still in a very chaotic state. But these two kinds of theories have been often confounded with one another in the popular and even in the scientific mind and the chaotic state of the latter has been carried over and credited to the former also; so that many seem to think that the whole subject of mountain-origin is yet wholly in air and without any solid the foundation.

I. FORMAL THEORY.

A true formal theory, keeping close to the immediate facts in hand, must pass gradually from necessary inferences from smaller groups, to a wider theory which shall explain them all.

Inferences from 1 and 2, i. e., Thickness and Coarseness of Sediments.—The thickness of mountain sediments, as we have seen, is greatest along the axis and grows less as we pass away from that line. Now where do we find lines of very thick sediments forming at the present time? The answer is: On sea bottoms closely bordering continents. The whole washings of continents accumulate very abundantly along shore lines and thin out sea-

ward. Mountains were therefore born of sea-margin deposits. This view is entirely confirmed by the character of mountain sediments. We have seen that these are coarsest near the crest, becoming finer and then changing into limestones as we pass farther and farther away from the crest. Now this is exactly what we find in off-shore deposits. They are coarse sands and shingle near shore, and then become progressively finer seaward, until in open sea beyond the reach of even the finest mechanical sediments, they are replaced by organic sediments which form limestones. It seems evident, therefore, that the place of a mountain-range before mountain-birth was a marginal sea-bottom receiving abundant sediment from a contiguous continental land-mass. This explains at once the usual position of mountains on the borders of continents. Here, then, is one important point gained.

But such enormous thickness as we often find would be impossible unless the conditions of sedimentation on the same spot were continually renewed by *pari passu* subsidence of the sea-bottom. And we do indeed find abundant evidence of such *pari passu* subsidence, not only at the present time in places where abundant sediments are depositing, but also in the strata of all mountain ranges. In the 40,000 feet thickness of Appalachian strata nearly every stratum gives evidence by its fossils, of shallow water, and often by shore marks of all kinds, of *very* shallow water. Therefore the place of mountains while in preparation, in embryo, before birth, *was gradually subsiding, as if borne down by the weight of the accumulating sediments*, and continued thus to subside until the moment of birth, when of course a contrary movement commenced. The earth's crust on which the sediments accumulated was bent into a great trough, or what Dana calls a Geo-Syncline. This is another important poined gained.

But let us follow out our logic. If the earth's crust yields under increasing weight of accumulating sediments, then ought it also to rise under the decreasing weight of eroded land surfaces. If it sinks by loading it ought also to rise by unloading.

And such indeed seems to have been the fact. For if all the strata which have been removed from existing plateaus and mountains were restored, it would make an incredible height of land. At least 10,000 to 12,000 feet have been carried away by erosion from the Colorado Plateau region and yet 8,000 feet remain. At least 30,000 feet have been worn away from the Uinta Mountains and yet 10,000 feet remain. Evidently there has been a rise *pari passu* with the lightening by erosion.

May we not then safely generalize? May we not conclude with Dutton that the earth in its general form and in its greater inequalities is in a state of gravitative equilibrium — that the earth is oblate spheroid, only because this is the form of gravitative equilibrium of a rotating body; that ocean basins and continental protuberances exist, only because the materials underlying the former are denser, and underlying the latter lighter than the average. It is true that the spheroid form of the earth and the sinking and rising of the crust by loading and unloading may be explained on the supposition that the earth is liquid beneath a thin crust, but to this view there are three fatal objections. 1. The cosmic behavior of the earth is that of a rigid solid. This I believe to have been demonstrated. 2. The existence of the present great inequalities of the earth would be impossible, except under the most improbable conditions. For example, if the earth be fluid then the crust must rest as a floating body. But if so, then, by the laws of floatation, for every continental protuberance on the upper side there must be a corresponding protuberance in reverse on the other side of the crust, and for every great plateau or mountain range there must be a corresponding plateau or mountain range in reverse. And taking the difference of specific gravity of the floating crust and the supporting liquid to be as great as that between ice and water, these reverse inequalities must be ten times as great as those at surface! Can we accept so violent an hypothesis? But (3) repeated experiments, especially very recent ones by Carl Barus,[1] prove that rocks

[1] Am. Journal, vol. 45, p. 1., 1893.

increase very notably in density in the act of solidification, so that a solid crust would undoubtedly break up and sink in a liquid of the same material. But how then are we to explain gravitative equilibrium in the case of a rigidly solid globe. I answer, by two suppositions. 1. That the earth, though rigid as glass or even steel, to *rapidly* acting force, yet yields *viscously* to heavy pressuse *over large areas* and acting *for a long time*. A solid globe of glass six feet in diameter will very perceptibly change form under its own weight. How much more the earth under its own gravity. This completely explains the oblateness of the earth even if solid throughout and had never been liquid at all. The earth, though rigid, behaves like a very stiffly viscous body ; like, for example, the ice of glaciers though very much more stiffly viscous. This viscosity would not at all interfere with its rigidity under the tide-generating influences of the sun and moon — for these are far too rapidly acting.

2. The second supposition necessary is, that the earth is *not* absolutely *homogeneous* either in density, or in conductivity for heat, that in secular cooling and contraction the denser and more conductive areas, cooling and contracting faster, went down and became the ocean basins, while the lighter and less conductive areas were left as the more prominent land surfaces. And thus to-day the ocean basins are in gravitative equilibrium with the continental areas, because in proportion as oceanic radii are shorter are the materials also *denser;* and in proportion as the continental radii are *longer*, are the materials also specifically *lighter*. This condition of gravitative equilibrium Dutton calls *Isostasy*.

Thus then the great inequalities of the earth, constituting ocean basins and continental surfaces, are the result of *unequal radial descent of the earth's surface* by contraction in its secular cooling. This is by far the most satisfactory theory of these *greatest* inequalities.

In thus following the phenomena of Isostasy to their logical conclusion, we seem to have gone beyond the limits of our subject, which is the *theory of mountains:* but the close connection which probably exists between the cause of continents and the

cause of mountains justifies the digression, if such it may be called.

Inferences from 3 and 4, Folding and Cleavage.—Still adhering closely to observed facts, there are some necessary inferences from folded structure and cleavage. These structures are indisputable proofs that mountain strata have been subjected to enormous *lateral* pressure at right angles to the trend of the axis, by which the whole mass has been mashed together horizontally. But such horizontal mashing must of necessity produce corresponding up-swelling along the line of yielding. In a word, it is evident that mountains have been uplifted largely, at least, if not wholly, by horizontal mashing. The only question that remains is, Is lateral mashing alone sufficient to produce the highest mountains? Let us see.

The amount of uplift in such cases would depend on two things, viz., the thickness of the strata and the amount of mashing. Now, as already shown, mountain sediments are 30,000, 40,000 and even 50,000 feet thick. The amount of mashing in many mountains is almost incredible. In the Appalachian it is so extreme that in one place, according to Claypole, ninety-six miles of the original sediments have been crowded into sixteen miles, and the shortening of the whole Appalachian breadth is estimated as eighty-eight miles.[1] In the Alps the shortening is estimated by Heim at seventy-two miles or one-half the original breadth of the sediments.[2] In a word, we may without exaggeration say that, in great mountains, the original space is to the folded space as two to one, or even three to one. Now a crushing of 30,000 feet of sediments into one-half their original space would double their thickness, which is equivalent to a clear elevation of 30,000 feet. But strata are 40,000 and even 50,000 feet thick. Evidently then this method alone is sufficient to account for the highest mountains in the world, even allowing for the enormous erosion which they have suffered.

The same is equally shown by the phenomena of slaty cleav-

[1] Amn. Natst. Vol. 19, p. 257.

[2] HEIM: Archives des Sciences, Vol. 64, p. 120, 1878.

age so often associated with folded structure. Slaty cleavage, as has been demonstrated by experiment, as well as by field observation, is produced by a mashing together of the whole rocky mass in a direction at right angles to the cleavage plane and a corresponding extension in the direction of the *dip* of these planes. Now since the cleavage dip is usually nearly or quite vertical, this means a mashing together *horizontally* and a proportionate extension *vertically*. The amount of mashing together horizontally and extension vertically has been in many cases somewhat accurately estimated. In this case also, as in folding, we have evidence of a mashing of two or even three into one and a corresponding extension vertically of one into two or even three. This amount of extension affecting thick strata is sufficient to account for the highest mountains in the world without resorting to any hypothetical force pushing upward from beneath.

There seems therefore to be no reasonable doubt that *mountains are formed wholly by lateral crushing with proportionate upswelling*. This is a very important point gained. Let us hold it fast. This brings me naturally to the next point.

Inferences from 5 and 6, Granitic Axis and Asymmetric Form.— A granitic or metamorphic axis is a very general, though not a universal, characteristic of mountains. The old idea (still held by some) was that fused matter was pushed up through and appeared above, the parted strata along the crest as the granite axis, lifting the strata, as it were, on its shoulders to form the slopes. But it must be observed that the axis is often only metamorphic, not granitic, and moreover that some mountains are composed wholly of folded strata alone. If, therefore, we regard granite as often only the last term of metamorphism, we may more properly speak of the axis of mountains as metamorphic. If so, then it is not necessary to suppose any vertical uprising of fused matter by volcanic forces at all. On the contrary, we would explain the axis thus :

It is evident that accumulating sediments must cause corresponding rise of the interior heat of earth toward the surface so as to invade the lower parts of the sediments and their included

water. Now it is well known from the experiments of Daubreé and others, that in the presence of water, even in small quantities, rocks become softened and even hydrothermally fused at the very moderate temperature of 400° to 800° F. It is certain then that such thickness of sediments as we know accumulated in preparation for mountain birth, must have been softened to a degree proportionate to the thickness, and therefore perhaps semi-fused or even fused in their lower parts along the line of *thickest* deposit, and therefore of greatest subsequent elevation. On cooling after elevation, this sub-mountain fused or semifused matter would form a granitic or metamorphic *core* beneath the highest part. The appearance of this core as an axis along the crest is the result not of up-thrust but of *subsequent erosion* greatest along this line.

And this, in its turn, furnishes a key to the location of mountains along lines of thick sediments. For not only the lower parts of such sediments but also the sea-floor on which they are laid down would be hydrothermally softened or even fused. Thus would be determined a line of *weakness*, and therefore a line of yielding to lateral thrust, and therefore also a line of crushing, folding, and upheaval. The folding and the upswelling and the metamorphism would be greatest along the line of thickest sediments and become less as we pass away from that line. In extreme cases, however, the firmer lateral portions might be jammed in under the softer central portions, on one or both sides, and give rise to the Fan-structure character of complexly folded mountains. Or again, in such cases the folds might be pushed clean over and broken at the bend, and then the upper limb slidden over the lower limb even for miles, forming the wonderful thrust-planes of the Alps, the Appalachian and the Rocky Mountains, already described. Thus the phenomena under (5) is completely explained.

But mountains are usually asymmetric, the crest being on one side. This is explained as follows: Sedimentary accumulations along shore lines are thickest *near* shore (though not *at* shore) and thin out slowly seaward. The cilinder-lens formed by sedimentation is not symmetric, its thickest part being near one side, and

that the shore side. This thickest line, as we have seen, becomes the crest, which therefore is asymmetrically placed on the land-side or side from which the sediments were derived. The over-folding on the contrary is to the sea-ward.

SUMMARY STATEMENT OF THE FORMAL THEORY.

We may therefore group all these inferences and sum up our view of the mode of mountain formation thus:

1. Mountain ranges, while in preparation for future birth, were marginal sea-bottoms receiving abundant sediment from an adjacent land-mass and slowly subsiding under the increasing weight. 2. They were at first formed, and continued for a time to grow, by *lateral pressure* crushing and folding the strata together horizontally and swelling them up vertically along a certain line of easiest yielding. 3. That this line of easiest yielding is determined by the hydrothermal softening of the earth's crust along the line of thickest sedimentation. 4. That this line, by softening, becomes also the line of greatest metamorphism; and by yielding, the line of greatest folding and greatest elevation. But (5) when the softening is very great sometimes the harder lateral strata are jammed in under the crest, giving rise to Fan-structure, in which case the most complex foldings may be near but not at the crest. Finally (6) the mountains thus formed will be asymmetric because the sedimentary cilinder-lenses from which they originated were asymmetric.

SOME EXAMPLES ILLUSTRATING.

It is hardly necessary to enforce these views by illustrative examples. They at once arise in the mind of every geologist. But there are those in this audience who are not geologists. I therefore select a few examples among our own mountains.

1. *Appalachian.* It is well known that during the whole Palæozoic, the region now occupied by the Appalachian was the eastern marginal bottom of the great interior Palæozoic Sea, receiving abundant sediments from an eastern land mass of Archæan rocks, which then extended far beyond the present limits of the continent and whose western coast-line was a little to the east

of the present Appalachian crest. The sediments along this marginal sea-bottom increased in thickness during Cambrian, Silurian, Devonian and Carboniferous (with some changes of Physical Geography, but without greatly changing the line of sedimentation) until 40,000 feet thickness was reached. Such thickness, of course, could not be attained without *pari passu* subsidence. We have additional evidence of this in shallow water fossils and even shore marks at many levels in the series. At the end of the coal period, when 40,000 feet had accumulated, the increasing softening along the line caused it finally to yield to horizontal thrust; the whole mass of strata was crumpled together and swelled up along the line of sedimentation and the Appalachian Range was born. The same forces which caused its birth continued to cause its *growth* for a long time. Subsequent erosion has sculptured it into its present form, but *has not exposed its granite core*. The crest is on the east or landward side, as we should expect, and the overfolds are to the west or toward the sea of that time. This is perhaps the most typical example we have.

2. *Sierra*.—If it were not for a subsequent movement so late as the beginning of the Quaternary, which greatly modified its form, the Sierra too would be a typical range. During the whole Palæozoic and the greater part of the Mesozoic the place now occupied by the Sierra was the eastern marginal bottom of the Pacific, receiving sediments from a continental land-mass in the present Basin region. The shore line changed somewhat at the end of the Palæozoic, but the Sierra region maintained a sea bottom position. At the end of the Jura, when an enormous thickness had accumulated, the increasing softening of the crust determined a yielding to lateral thrust and consequent formation of the range. Subsequent erosion has completely removed the strata from the crest and exposed the granitic core as an axis[1]. This axis is here also on the landward side, and the overfolds are

[1] Sierra granite is not Archæan as has been asserted by some, nor does it all antedate the birth of the range. This is proved (1) by the gradation traceable between slates and granites, and (2) by the fact stated by Whitney, by Fairbanks, and by Diller —that the granite in many places penetrates the slate as veins.

to the seaward as in the Appalachian. The erosion of the Cretaceous and Tertiary times probably cut down the Sierra to very moderate proportions and reduced it to an almost senile condition. At the end of the Tertiary a great fault and bodily uplift of the whole Sierra block on its east side transferred its crest to the extreme eastern margin, greatly increasing its height and rejuvenating its erosive vigor.

3. *Coast Range.*—The formation of the Sierra transferred the coast line westward of that range and the present place of Coast Range became marginal sea-bottom, receiving sediment from a now greatly increased land-mass. This continued until the end of the Miocene when the Coast Range was similarly formed.

We might multiply examples, but these are deemed sufficient to illustrate the principles.

MINOR PHENOMENA.

We have given only the most fundamental phenomena, *i.e.*, those which reveal the mode of origin, and upon which, therefore, a true theory must be founded. But all other minor phenomena associated with mountains are well explained by the view above presented and their explanation confirms the view. For example:

1. *Eruptive Phenomena.*—We have seen that beneath a mountain, before and at the time of its formation, there is a deep-seated core of liquid or semiliquid matter. Also it is evident that the strong foldings of the strata in the act of mountain formation must produce fissures parallel to the folds and to the mountain axis, and that these fissures may reach down to the submountain liquid matter. In the act of mountain formation, therefore, the submountain liquid must be squeezed into the fissures forming dikes, or through the fissures and poured out on the surface as great lava floods, covering sometimes thousands of square miles. In most cases subsequent erosion has swept these overflows clean away leaving only their roots as intersecting dikes. Only the most recent still remain. On these great fissure-eruption lava-fields, ordinary volcanic or crater eruptions continue for ages

after the mountain formation ceases. In these, however, materials are ejected not by mountain-making forces, but by the elastic force of vapor from percolating waters. All these eruptive phenomena are, therefore, associated with mountain ranges.

2. *Faults.*—In folding, and especially overfolding, the strata are, of course, often broken and the upper wall of the fissure is pushed over the lower wall by horizontal thrust often thousands of feet, forming reverse faults and so-called thrust planes. Hence this style of faults are everywhere associated with strongly folded rocks, and, therefore, with mountains, and are indisputable evidence of horizontal crushing. In other places than mountains, and in horizontal or gently folded rocks, the other style of faults, *i. e.*, normal faults, are more common.

3. *Mineral Veins.*—The filling of fissures at the moment of formation with fused matter constitute dikes; but if not so filled, they are afterwards filled by a slow process of deposit from circulating waters and then they form mineral veins. These, therefore, are also common in mountains.

4. *Earthquakes.*—Again, the immense dislocations of strata which we find in faults did not occur all at once, but slowly through great lapse of time; and yet on the other hand not by uniform slipping, but by jerks, a little at a time. Every such readjustment of the walls of a fissure, whether by increasing lateral pressure (reverse faults) or by gravity (normal faults), gives rise to an earthquake. Earthquakes, therefore, although not confined to, are most common in mountain regions, especially if the mountains are still growing.

Thus, leaving out the monoclinal type which seems to belong to different category, all the phenomena, major and minor, of structure and of occurrences connected with mountains, are well explained by the theory of *lateral pressure* acting on lines of thick sediments accumulated on marginal sea-bottoms and softened by invasion of interior heat. This view is therefore satisfactory as far as it goes, and brings order out of the chaos of mountain phenomena. It has successfully directed geological investigation in the past and will continue to do so in the future.

But there still remains the question: "*What is the cause of the lateral pressure?*" The answer to this question constitutes the *physical theory*.

Thus far I suppose there is little difference of opinion. I have only tried to put in clear condensed form what most geologists hold. But henceforward there are the most widely diverse views and even the wildest speculations. But let us not imagine, on that account that we have made no progress in the science of mountain-origin. The *formal theory* already given is really for the geologist by far the most important part of the theory of mountain-origin. For I insist that for the geologist *formal* theories are usually more important than *physical* theories of geological phenomena. That slaty cleavage is the result of a mashing of strata by a force at right angles to the cleavage-planes, is of capital importance to the geologist, for it is a guide to all his investigations. To what property of matter this structure is due is of less importance to him, though of prime importance to the physicist. That the phenomena of the drift is due to the former existence of a moving ice-sheet is the one thing most important to the geologist, guiding all his investigations. Whether this ice-sheet was caused by geographical or astronomical changes is a question of wider but of less direct interest to him. So in the case of mountain ranges, the most important part of the theory is their origin by lateral pressure under the conditions given above. The *cause* of the lateral pressure, though still of extreme interest, is certainly of less immediate importance in guiding investigations.

PHYSICAL THEORIES.

The most obvious view of the cause of lateral pressure refers it to the *interior contraction of the earth*. This may be called the

"CONTRACTIONAL THEORY."

This theory is so well known that I will give it only in very brief outline. It assumes that the earth was once an incandescent liquid and has cooled and solidified to its present condition. At first it cooled most rapidly at the surface and must have fissured

by tension. But there would inevitably come a time when the surface being substantially cool and moreover receiving heat also from the sun, its temperature would be fixed or nearly so, while the incandescent interior would be still cooling and contracting. Such has probably been the case ever since the commencement of the *recorded* history of the earth. The hot interior now cooling and contracting more rapidly than the cool crust, the latter following down the ever shrinking nucleus would be thrust upon itself by lateral pressure with a force which is simply irresistible. If the crust were ten times, yea one hundred times more rigid than it is, it must yield. It does yield along the lines of greatest weakness, *i. e.*, along marginal sea-bottoms as already explained. As a first attempt at a physical theory, it seems reasonable, and therefore, until recently, has been generally accepted.

OBJECTIONS TO THE CONTRACTIONAL THEORY.

It is well known that American geologists have taken a very prominent part in the study of mountain structure and mountain origin. So much so indeed that the *lateral pressure theory* in the form given above and interior contraction as its cause, have sometimes been called the "American theory." It is also well known that my name, among others, especially Dana's, has been associated with this view. All I claim is to have put the whole subject, especially the formal theory, in a clearer light and more consistent form.[1] The formal theory I regard as a permanent acquisition. The contractional theory may not be so. It is natural, from my long association with it, that I should be reluctant to give it up. But I am sure that I am willing to do so if a better can be offered. We all dearly love our own intellectual children, especially if born of much labor and thought; but I am sure that I am willing, like Jephtha of old, to sacrifice, if need be, this my fairest daughter on the sacred altar of Truth. Objections have recently come thick and fast from many directions. Some of these

[1] "Theory of the Formation of the Great Features of the Earth's Surface." Am. Journal, Vol. 4, pp. 345 and 460, 1872, and also "Structure and Origin of Mountains," Vol. 16, p. 95, 1878.

I believe can be removed ; but others perhaps cannot in the present condition of science, and may indeed eventually prove fatal. Time alone can show. I state briefly some of these objections.

1. Mathematical physicists assure us that on any reasonable premises of initial temperature and rate of cooling of the earth, the *amount* of lateral thrust produced by iuterior contraction would be wholly insufficient to account for the enormous foldings.[1] Let us admit—surely a large admission—that this is so. But this conclusion rests on the supposition that the whole cause of interior contraction is *cooling*. There may be other causes of contraction. If cooling be insufficient, our first duty is to look for other causes. Osmund Fisher has thrown out the suggestion (a suggestion by the way highly commended by Herschel) that the enormous quantity of water vapors ejected by volcanoes and the probable cause of eruptions is not meteoric in origin as generally supposed, but is original and constituent water occluded in the interior Magma.[2] Tschermak has connected this escape of constituent water from the earth with the gaseous explosions of the sun.[3] Is it not barely possible that we may have in this an additional cause of contraction, more powerfully operative in early times but still continuing? See the large quantity of water occluded in fused lavas to be "*spit out*" in an act of solidification! But much still remains in volcanic glass which by refusion intumesces into lightest froth. Here then, is a second possible cause of contraction. If these two be still insufficient, we must look for still other causes before rejecting the theory.

2. Again: Dutton[4] has shown that in a *rigid earth* it is impossible that the effects of interior contraction should be concentrated along certain lines so as to form mountain ranges, because this would require a shearing of the crust on the interior. The yield-

[1] Cam. Phil. Trans. Vol. XII., Part II., Dec. 1873.

[2] Cambridge Phil. Trans. Vol. XII., Part II., Feb. 1875. Physics of the Earth's Crust, p. 87.

[3] Geol. Mag. Vol. IV., p. 569, 1877.

[4] Am. Jour. Vol. VIII., p. 13, 1874. Penn. Monthly, May, 1876.

ing according to him would be evenly distributed everywhere and therefore imperceptible anywhere. This is probably true, and therefore a valid objection in the case of an earth *equally rigid in every part.* But if there be a sub-crust layer of liquid or semi-liquid or viscous, or even more movable or more unstable matter, either universal or over large areas, as there are many reasons to think, then the objection falls to the ground. For in that case there would be no reason why the effects of general contraction should not be concentrated on weakest lines as we have supposed.

3. But again: it has been objected that the lines of yielding to interior contraction ought not to run in *definite* directions for *long distances,* but irregularly in *all* directions. I believe we may find the answer to this objection in the principle of flow of solids under very slow heavy pressure. The flow of the solid earth, under pressure in *many* directions, might well be conceived as being deflected to the direction of least resistance, *i. e.*, of easiest yielding.

4. But again: it will be objected that the amount of circumferential shortening necessary to produce the foldings of some mountains is simply incredible; for it would disarrange the stability of the rotation of the earth itself. According to Claypole, in the formation of the Appalachian range, the circumference of the earth was shortened eighty-eight miles and in the formation of the Alps seventy-two miles. Now this would make a decrease of diameter of the earth of twenty-eight miles in the one case and twenty-three in the other. This would undoubtedly seriously quicken the rotation and shorten the day. This seems indeed startling at first. But when we remember that the tidal drag is all the time retarding the rotation and lengthening the day and much more at one time than now, we should not shrink from acceptance of a counteracting cause hastening the rotation and shortening the day, and thus giving stability instead of destroying it. We must not imagine that there would be anything catastrophic in this readjustment of rotation. Mountains are not formed in a day nor in a thousand years. It requires

hundreds of thousands of years, or even millions of years—if physicists allow us so much.

The objections thus far brought forward, though serious, are by no means unanswerable. But there is one brought forward very recently which we are not yet fully prepared to answer and may possibly prove fatal.

5. *Level of No Strain.*—Until recently the interior contraction of the earth was considered only roughly and without analysis. It was seen that the surface was already cool and its temperature fixed while the interior was still hot and cooling; and therefore that the exterior must be thrust upon itself and be crushed. But the phenomena are really far more complex than at first appears. It is necessary to distinguish between two kinds of contraction to which the interior layers are subjected, viz., radial and circumferential. If there were radial contraction only, then undoubtedly every concentric shell as it descended into smaller space would be crushed together laterally. But there is for all layers, except the surface, also a circumferential contraction, and this would have just the opposite effect, *i.e.*, would tend to stretch instead of crush. Therefore wherever the decrease of space by descent is greater than the circumferential contraction, there will be crush, and where the circumferential contraction is greater than the decrease of space by descent, there will be tension and tendency to crack. There would be no *real* cracking, only because incipient cracks would be mashed out or rather prevented by superincumbent pressure. Where these two are equal to one another, there will be no strain of any kind. There is a certain depth at which this is the case. It is called the "*level of no strain.*" To Mellard Reade is due the credit of first calling attention to this important principle.

Let us analyze the principle more closely. It is admitted that at the surface there is no contraction of any kind. It is also calculated that contraction of all kinds cease at depth of 400 miles. It is believed farthermore that commencing 400 miles below the surface and coming upward the contraction increases very slowly from zero to a maximum at the depth of 70 miles

and then decreases again more rapidly to zero at the surface. This is shown in diagram, Fig. 1. In this figure the curve represents the relative rate of contraction whether radial or circumferential of the several layers. We use it, however, only to represent the latter. For in considering the radial contraction, it is not the relative rate of the several layers that immediately concerns us, but their rate of *radial descent.* Now this is a *summation series* and therefore increases to the very surface, but at different rates of increase. The law of increase of radial descent as we come toward the surface is shown in diagram, Fig. 2[1] in which the rate of increase is greatest at seventy miles, just where the curve changes from concavity to convexity. If now we superpose these two diagrams the depth *a* at which the two curves,

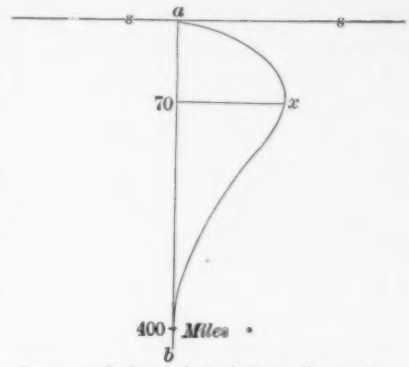

FIG. 1. *s s*=Surface; *a b*=depth along radius; *a x b*=curve of contraction.

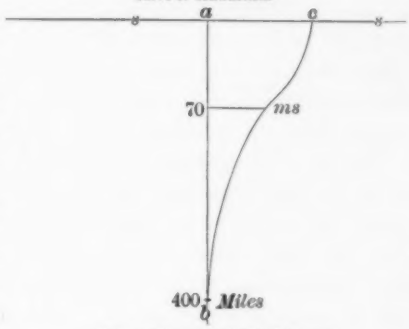

FIG. 2. *c b*=curve of radial descent.

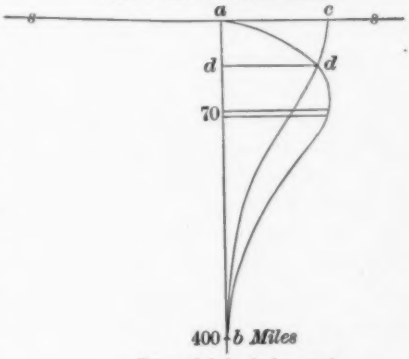

FIG. 3. *d d*=level of no strain.

[1] I have taken these figures from Claypole, but modified this one so as to make it a truer representation of the law.

viz., that of circumferential contraction and that of radial descent, intersect, is the level of no strain.

Now laborious calculations have been made by Davison, Darwin and Fisher to determine the depth of this level of no strain. All make it very superficial. Davison, taking an initial temperature of 7000° F., makes it five miles below the surface. Fisher, on the same data, only two miles, and with an initial temperature of 4000° only *0.7* of a mile. It is easy to see that if this be true, the amount of lateral thrust must be small indeed.

Now undoubtedly there is a true principle here which must not hereafter be neglected, but it is almost needless to say that these quantitative results are in the last degree uncertain. The calculations are, of course, based on certain premises. These are a uniform initial temperature of say 7000° F., a time of cooling, say 100 or 200 millions of years, and a certain rate of cooling under assumed conditions. The depth of the level of no strain increases with the time and is still going downward. In a word, in a question so complex both mathematically and physically and in which the data are so very uncertain, every cautious geologist, while freely admitting the soundness of the principle, will withhold assent to the conclusions. Huxley has reminded us that the mathematical mill, though a very good mill, cannot make wholesome flour without good wheat. It grinds indifferently whatever is fed to it. It has been known to grind peas cods ere now. It may be doing so again in this case. Let us wait.

But besides withholding assent and waiting for more light, I may add that these calculations, of course, go on the supposition that the whole contraction of the earth is due to loss of heat; but as we have already said, it may be due also to loss of constituent water. This would put an entirely different aspect on the subject.

ALTERNATIVE PHYSICAL THEORIES.

I have given the objections to the contractional theory frankly and I think fairly. They are undoubtedly serious. Let us see what has been offered it its place.

I. READE'S EXPANSION THEORY.

This, the most prominent among alternative theories, was first brought forward in Mr. Reade's book on "Origin of Mountain Ranges." Although I have carefully read all that Mr. Reade has written on the subject, I find it difficult to get a clear idea of his views. But, as I understand it, it is in outline as follows: (1) Accumulation of sediments off shore and isostatic subsidence of the same. (2) Rise of isogeotherms and heating of the whole mass of sediments and of the underlying crust in proportion to the thickness of the sediments. (3) *Expansion* of the whole mass in proportion to the rise of temperature. If there were no resistance, this expansion would be in all directions (cubic expansion). (4) But since the containing earth will not yield to expansion laterally, this lateral expansion is satisfied by *folding*, and this in turn produces *vertical upswelling*. Thus the whole *cubic expansion is converted into vertical expansion*, which is therefore three times as great as the linear expansion in any one direction. (5) Elevation would of course anyhow be greatest along the line of thickest sediment; but this by itself would not be sufficient to produce a mountain. (6) But farther—and here the theory is more obscure—there is a concentration of the effects of expansion, along a comparatively narrow line of thickest sediment, by a *flow* of the hydrothermally plastic or even liquid mass beneath, *toward* this central line and then *upward* through the parted strata, folding these back on either side and appearing at the crest as the granitic or metamorphic axis. (7) In his latest utterances he seems to adopt the view of Reyer, viz., that the uplifted strata slide back down the slope, producing the enormous crumpling so often found, and exposing a wider area of granite axis. (8) From the same liquid mass which lifts the mountain, come also the great fissure eruptions and the volcanoes.

Mr. Reade makes many experiments to determine the linear expansion of rocks, and he thinks that these experiments show that when cubic expansion is converted into vertical expansion and this again concentrated along a line one-fourth to one-fifth the whole breadth of the expanding mass, it would explain the

elevation of the highest mountains. But still he seems uncertain if it be enough. In fact, he declares that if it were not for another factor yet unmentioned, he probably would never have brought forward the theory at all.

(9) This factor is *recurrency of the cause and accumulation of the effects.* And here the previous obscurity becomes intensified. I have read and re-read this part without being able wholly to understand him. He seems to think that when expansion had produced elevation, the mountain thus formed would not come down again by cooling and contraction; but on the contrary would *wedge up by normal faulting and set in its elevated position.* Afterward, by new accumulation of heat, another elevation and setting would take place and the mountain grow higher, and so on indefinitely, or until the store of heat is exhausted. Therefore he characterizes his theory as that of "*Alternate expansion and contraction,*" or again as that of "*Cumulative recurrent expansion.*"

Such is a very brief, perhaps imperfect, but I hope fair outline of Reade's theory. It seems to me that there are fatal objections to it. These I now state.

Objections.—1. The first is *inadequacy* to account for the enormous foldings of the mountains especially when there is no granite axis to fold back the strata. It is true that Mr. Reade makes comparison between his own and the contractional theory in this regard, and seems to show the much greater effectiveness of his own. This may be true if we accept his premises and compare *equal areas* in the two cases. But the contractional theory *draws from the whole circumference* of the earth and *accumulates* the effects on one line, while in Reade's theory the expansion is, of course, very *local.*

2. But the fatal objection is that brought forward by Davison. It is this: sedimentation cannot, of course, increase the sum of heat in the earth. Therefore the increased heat of the sediments by rise of isogeotherms, *must be taken from somewhere else.* Is it taken from below? Then the radius below must contract as much as the sediments expand and therefore there will be no elevation. Is it taken from the containing sides? Then

the sides must lose as much as the sediments gain, and therefore must contract and make room for the lateral expansion, and therefore there would be no folding and no elevation. I do not see any escape from this objection.

Thus it seems that Reade's theory cannot be accepted as a substitute. Is there any other?

II. DUTTON'S ISOSTATIC THEORY.[1]

Dutton's discussion of isostasy is admirable, but his application of it to the origin of mountains is weak. The outline is as follows:

Suppose a bold coast line, powerful erosion and abundant sedimentation. The coast rises by unloading and the marginal sea-bottom sinks by loading. Now if isostasy is perfect, there will be no tendency to mountain formation. But suppose a piling up of sediments, but—on account of earth rigidity—without immediate compensatory sinking, and a cutting down of coast land without compensatory rising. Then *there would be an isostatic slope toward the land.* And the accumulated and softened sediments would *slide landward, crumpling the strata and swelling them up into a mountain range.*

The fatal objection to this view is that complete isostasy is necessary to renew the conditions of continued sedimentation and therefore to make thick sediments, otherwise the sediments quickly rise to sea-level and stop the process of sedimentation at that place. But it is precisely a *want* of complete isostasy which is necessary to make an isostatic slope landward. Dutton refers to Herschel as having suggested a similar cause of strata crumpling and slaty cleavage[2]; but the principles involved in the two cases are almost exactly opposite. Herschel supposes sediments to slide down steep *natural* slopes of sea-bottoms and therefore *seaward.* Dutton supposed sediments to slide *up* natural, though *down* isostatic slopes, *landward.* Herschel's is a theory of strata-

[1] Phil. Soc. of Washington, Bull. Vol. XI, pp. 51–64, 1889.

[2] Phil. Mag., Vol. 12, 197, 1856.

crumpling and slaty cleavage; Dutton's a theory of mountain formation.

There has been no attempt to carry this idea of Dutton's to quantitative detail. It was probably thrown out as a suggestion in mere despair of any other explanation, for he had already repudiated the contractional theory. But the least reflection is sufficient to convince that such slight want of complete isostatic equilibrium as may sometimes occur would be utterly inadequate to produce such effects.

III. REYER'S GLIDING THEORY.[1]

Prof. Reyer has recently put forward certain views fortified by abundant experiments on plastic materials. His idea in brief seems to be this: Strata are lifted and finally broken through by up-rising fused or semi-fused matters and these appear above as the granitic axis. As the axis rises, the strata are carried upward on its shoulders, until *when the slope is sufficiently steep the strata slide downward crumpling themselves into complex folds* and exposing the granitic axis in width proportioned to the amount of sliding.

No doubt there is much value in these experiments of Reyer, and possibly such gliding does indeed sometimes take place in mountain strata and some foldings may be thus accounted for. But the great objections to this view are (1) that there is no adequate *cause* given for the *granitic uplift*, and (2) that it utterly fails to account for the complex foldings of such mountains as the Appalachian and Coast Range *where there is no granite axis at all*. Reade, indeed, holds that the Piedmont region is the granite axis of the Appalachian, and that the original strata of the eastern slope are now buried beneath the sea. But American geologists are unanimous in the belief that the shore line of the great interior Palæozoic sea was but a little east of the Appalachian crest and the sea washed against land of Archæan rocks extending eastward from that line.

[1] Nature, Vol. 46, p. 224, 1892, and Vol. 47, p. 81, 1892.

CONCLUSION.

After this rapid discussion of alternative theories in which we have found them all untenable, we return again to the contractional theory, not indeed with our old confidence, but with the conviction that it is even yet the best working hypothesis we have.

JOSEPH LE CONTE.

ON THE MIGRATION OF MATERIAL DURING THE METAMORPHISM OF ROCK-MASSES.

THE researches of numerous geologists during the last two decades have placed at our disposal a large amount of information respecting the metamorphism of rocks, and from the facts thus collected we are now in a position to draw conclusions which we may expect to have a wide application. The important changes that affect the character of rock-masses divide roughly into two classes.

First, there are those dependent on meteoric agencies. These changes, though not necessarily superficial in the ordinary sense, are due in the first place to the action of circulating waters in communication with the atmosphere, and as a rule they involve the addition or subtraction of various ingredients or the transference of material from one place to another. The ordinary "weathering" effects illustrate the removal of alkalies and silica, the addition of water, oxygen, carbonic acid, etc. We must also include the processes which have given rise to many crystalline limestones and quartzites, serpentine-rocks, dolomites, ironstones, and jaspers, and even (as appears from Van Hise's researches in the Penokee region) some mica-schists and fine-grained gneisses. The characteristic of almost all these transformations is that they are metasomatic as well as metamorphic.

Secondly, we have those transformations more usually understood by the term metamorphism : viz., dynamic metamorphism, due to high pressure operating upon rock-masses, and thermal metamorphism, due to high temperature, whether produced by an intrusion or by the mechanical generation of heat. In these various cases of metamorphism proper, metasomatism is rather the exception than the rule. I shall deal here with thermal metamorphism only, and shall draw my data chiefly from the rocks surrounding the large igneous intrusions of the English Lake District, investigated by Mr. Marr and myself, but the conclusions are confirmed in other areas.

Metasomatic changes are known to take place during thermal metamorphism as regards the *volatile* constituents of the rocks affected. A (usually partial) loss of water and the elimination (under proper conditions) of carbonic acid from carbonates are instances of this; a more special case is the accession of boric and hydrofluoric acids near the contact of metamorphosed rocks with certain acid intrusives. Several observers have recorded a transference of other materials (silica and soda) from an invading igneous magma to the neighboring rocks, but such a phenomenon seems to be of uncommon occurrence, and to be confined to the immediate vicinity of the contact. Apart from the exceptions noted, there is every reason to believe that thermal metamorphism involves no alteration in the bulk-analysis of the rocks affected. Whatever part water may play in the various chemical changes that are set up, it does not (as in atmospheric metamorphism) act as a medium to transfer material to or from the rocks in question.

I believe that we can go further, and assert that within the mass of a rock undergoing thermal metamorphism any transference of material (other than volatile substances) is confined to extremely narrow limits, and consequently that, for a given temperature of metamorphism, the mineral formed at any point depends only on the chemical composition of the rock-mass within a certain very small distance around that point. Illustrations of this principle, as stated in the latter form, are familiar to all who have studied cases of "contact metamorphism":[1] they are very striking when some of the constituent substances of the original rock were, by weathering or otherwise, locally aggregated prior to metamorphism. By studying such cases we can not only verify the principle here laid down, but also arrive at an estimate of the actual limits within which interchange of material has taken place.

An excellent test-case is afforded by rocks containing calcite. It is well known that impure calcareous rocks are readily metamorphosed by heat into rocks rich in lime-silicates, with total

[1] Compare Bull. Geol. Soc. Amer. (1891) vol. iii., pp. 16–22.

elimination of the carbonic acid, while pure limestones or dolomites, under the same conditions, merely recrystallize without chemical change. In other words, the carbonates are decomposed in thermal metamorphism only in the presence of silica in some available form to take the place of the carbonic acid. Interesting illustrations of this are given by some of the rocks which have come under our notice.[1] The Strap granite in Westmoreland metamorphoses certain basic lavas containing amygdules of various dimensions, many of which were occupied, prior to the metamorphism, by calcite. Near the granite the smallest of these calcite-amygdules are converted into various silicates rich in lime, the silica having been derived from decomposition-products lining the original vesicles or from the immediately adjacent portion of the rock. In the larger metamorphosed amygdules, on the other hand, only the outer layers are transformed into lime-silicates, the interior still consisting of calcite; which, however, has recrystallized during the metamorphism, as is proved by its moulding the silicates and being penetrated by needles of actinolite, etc. Analogous appearances characterize veins and lenticles of calcite in shales and the converse case of argillaceous nodules imbedded in pure limestones and dolomites. The conclusion is that carbonic acid is displaced from the calcite only when there is in the immediate neighborhood either free silica or some substance capable of furnishing silica. Where calcite and quartz have recrystallized side by side in a metamorphosed rock, they are always separated by some one or more lime-bearing silicates, but their distance apart may be very small, and we deduce that the migration of silica to take the place of carbonic acid has been restricted to extremely narrow limits. In some highly altered rocks the distance is not more than one-twentieth of an inch.

The limit of migration of material no doubt increases with the temperature of metamorphism. This is well illustrated by some calcareous ashes or tuffs. At a considerable distance—say a thousand yards—from a large granite intrusion, the carbonic

[1] See especially Quart. Journ. Geol. Soc. (1893) vol. xlix., pp. 359–371.

acid is entirely expelled only from very fine-grained mixtures of calcareous and ashy materials: approaching the contact, the complete decomposition of the calcite is found to extend to successively coarser-grained rocks. Another line of inquiry is offered by the texture of the metamorphosed rocks themselves, of whatever lithological nature, in a district of metamorphism surrounding a large igneous intrusion. The size of the individual crystals of secondary minerals increases towards the contact with the intrusive rock: this may be taken to indicate that the migration of material within the mass of a rock undergoing metamorphism has more latitude when the temperature is higher. For various reasons, however, it would be unsafe to found numerical results upon such observations. The crystals of certain metamorphic minerals attain to considerable dimensions by virtue of their power of enclosing a large amount of foreign material; others, again, can apparently push aside solid impurities to make room for their own growth. The texture of the metamorphic rocks examined is still, however, in general accord with the conclusions reached by other methods of inquiry.

The question naturally arises whether the limit of migration of material is the same for different substances. On this point we have but little information. Among the various types of "spotted" rocks described in aureoles of metamorphism is one in which the spots are simply spaces free from the secondary brown mica abundant in the general mass of the metamorphosed rock. Since the iron compounds in the rock must originally have had a generally uniform distribution, the phenomena of the spots indicate a movement of ferrous oxide, and the radius of the spots gives a measure of the extreme limit of such movement. In the cases examined this is about one-twentieth of an inch, and we may infer that the greatest distance of migration of ferrous oxide has been about the same as that of silica at a similar temperature.

Not to insist unduly upon precise estimates, these and similar observations certainly tend to show that in thermal metamorphism no interchange of material takes place except between

closely adjacent points. The law that, apart from volatile constituents, the total chemical composition remains unchanged is true not only of the rocks in bulk, but of any individual cubic inch of the rocks. This might be followed out into various corollaries, of which I note only one, viz., that the greatest variety of metamorphic minerals is to be found in rocks which were the most heterogeneous prior to metamorphism. Such rocks are breccias and fault-breccias, etc., and especially basic igneous rocks more or less weathered before being metamorphosed.

ALFRED HARKER.

CAMBRIDGE, ENGLAND.

THE CORDILLERAN MESOZOIC REVOLUTION.

CERTAIN features connected with the occurrence of plutonic rocks on the western side of America suggest hypotheses which have an important bearing upon our general conceptions of the structural development of the continent. These features are but imperfectly and very partially recorded thus far in geological literature, owing to the vastness of the field and the meagre amount of investigation which has been devoted to it. Yet enough facts have been accumulated to have impressed the writer that they point to generalizations which have not yet been fully presented for the consideration of students of continental problems. To formulate these generalizations is the object of this brief note. It is not the purpose of the writer to add to the record of facts so much as to connote the more important of them and to suggest their cumulative significance.

The researches of Richardson[1] and Dawson[2] on the coast and islands of British Columbia have shown that the Cretaceous rocks of that region, ranging from the *Aucella* bearing horizon (Neocomian) to the Chico, repose upon a profoundly eroded complex of granite and metamorphic rocks. The disturbances which have affected these Cretaceous strata since their deposition have been of a local rather than of a regional character. They lie upon the old basement usually in but little disturbed attitudes, or are inclined at low angles, though occasionally they are faulted or sharply folded along certain lines of post-Cretaceous movement. The same condition seems to generally characterize the more elevated early Cretaceous rock of the British Columbian interior along the cañon of the Fraser river. Jurassic rocks have been described from British Columbia, but the Geological Survey of Canada has since come to the conclusion that these rocks are

[1] Reports of Progress, Geol. Survey of Canada, 1871–2, 1872–3, 1873–4, 1874–5, 1876–7.

[2] Report of Progress, Geol. Survey of Canada, 1878–9. Annual Report (New Series) Vol. II., 1886. Geol. Survey of Canada, Report B.

Cretaceous.[1] If the Jurassic exists on the west coast of British Columbia, it must occupy very limited areas or be involved in the pre-Cretaceous metamorphic complex. The fossils collected in the less altered portions of this complex by Richardson and Dawson, show the presence of Triassic and Carboniferous formations, but no undoubted Jurassic forms have yet been detected. It therefore seems that the erosion to which the region was subjected prior to the deposition of the Cretaceous was effected in Jurassic time. As Dawson has shown,[2] this erosion was of longer duration in the southern part of the province than in the northern, and the transgression of the Cretaceous sedimentation was from north to south.

The further studies of Dawson upon the pre-Cretaceous complex of granite and metamorphics have been fruitful of most interesting and important results. Prior to his researches the granite (and granite-gneisses) of the region were generally regarded as the equivalent of the Laurentian of the east. It was shown,[3] however, by him that the basement upon which the now metamorphic sedimentary and volcanic strata of the Vancouver series (Triassic, with probably some Carboniferous), was deposited is non-existent, and has been replaced by an immense mass of intrusive granite, which has absorbed by fusion all rocks below the present remnants of the Vancouver series, and has invaded the latter after the manner of an irruptive magma. This post-Triassic granitic batholite is of enormous dimensions. In the fall of 1890 the writer had an opportunity of examining it cursorily for a distance in a straight line of over five hundred miles, in and out of the fiords of the coast from Burrard Inlet to Alaska; and the granite is known to extend far northward into that territory. Its width may be placed at from sixty to one

[1] Sketch of the Geology of British Columbia, by G. M. Dawson, Geol. Mag., April and May, 1881.

[2] Am. Jour. Sci., Vol. xxxix., March, 1890.

[3] Annual Report (New Series), Vol. ii., 1886, Geol. Survey of Canada, Report B, pp. 10-13.

hundred miles. In the portion examined there are several masses or belts of schistose metamorphic rocks which have been sunk down into the granite, but they form a small proportion of the entire complex. The granite varies somewhat in mineralogical composition, texture, and structure, and is often distinctly gneissic locally. In places it is essentially hornblendic, in others it is micaceous. Notwithstanding these variations, which are common in most large granite masses, the granite seems to be a unit throughout, and the mass is certainly a very important factor in the epeirogeny of the west coast of America. Even should it be discovered by the closer scrutiny which science will certainly demand, that there are portions of an older granite terrane to be discriminated from the general mass, the conclusion will not be invalidated, that in the interval between the deposition of Triassic strata and the deposition of lower Cretaceous, the earth's crust was in this region invaded by an immense batholitic magma, hundreds of miles in extent, which absorbed a large part of the pre-Triassic basement, as well as a portion of the Triassic rocks themselves. This invasion of the crust by the British Columbian batholite seems to have conditioned a general and pronounced elevation of the coast. For the erosion which intervened before the deposition of the Cretaceous was possessed of a vigor only born of lofty mountains, removing the upper crust and cutting down deep into the congealed granite. The Cretaceous rocks were littoral deposits at the base of these lofty mountains. Thus was a great revolution wrought in the geology and physiography of the west coast of British Columbia in the interval between the Triassic and Cretaceous times.

Little is definitely known of the geology of the Olympic Mountains, but it is probable that the conditions which prevail on Vancouver Island, which is the northern extension of the range, hold good here, the Cretaceous rocks of the coast reposing upon the lower flanks of mountains which consist of a complex of granite and metamorphic rocks. These mountains are probably the least known portion of the United States, and they are mentioned here simply to indicate that important evidence

bearing upon the phenomena here discussed is likely to be found in that region.

In southern Oregon, on the line of the Southern Pacific Railway, the writer has on several occasions observed the eruptive contact of an extensive granite mass against sedimentary strata which have been mapped as "Auriferous slates," which are probably early Mesozoic or Carboniferous in age. The intrusion of the granite into the sedimentary rocks is unquestionable, the relations being well exhibited in the excellent exposures afforded by the railway cuttings.

In California the statements of Whitney[1] and the more recent writings of the geologists of the U. S. Geological Survey, Diller, Becker, Turner, and Lindgren, and of Mr. H. W. Fairbanks, seem to leave no room for doubt that a great part, probably the greater part, of the granitoid rocks of the Sierra Nevada is of Mesozoic age, and has invaded the now more or less altered sedimentary and volcanic rocks known as the "Auriferous slates," which range in age from the Silurian up to the Jurassic.

Here again we have clearly to deal with a granitic batholite which must, by absorption or otherwise, have replaced a large portion of the preëxisting lower rocks in the region affected. From the facts recorded by able and critical observers, this conclusion holds, notwithstanding the probability that there may also be remnants of an older granite to be discriminated from the Mesozoic mass. In the southern Sierra, as Becker has, with wise caution, pointed out, we approach the region of Archæan granite known in the Grand Cañon section. It would therefore be not at all remarkable to find these more ancient granites involved with the newer in the Sierra Nevada. But their presence could not affect the important fact of an invasion of the crust during middle Mesozoic time by an immense granitic batholite, which invasion without doubt had much to do with the metamorphism of the strata which survived the upward progress of the magma into the crust.

Here again the development of the batholite seems to have

[1] Geology of California, Vol. I. Auriferous Gravels.

conditioned the uplift and wide-spread disturbance which is freely recognized in geological literature as having occurred at the close of the Jurassic. Again we have, as in British Columbia, a wonderful dissolving of the ancient *status quo*, a revolution of no mean import, whether regarded merely as an historical event or in its bearing upon the general principles of epeirogeny. The important feature which distinguishes the group of facts observed in the Sierra Nevada from those in British Columbia is that in the former we have the Jurassic a part of the great assemblage of rocks invaded by the granite while in British Columbia these rocks are not known to exist. This difference, taken together with the probable fact that the pre-Cretaceous denudation of the Sierra was less profound than that of British Columbia, suggests a progressive development of the batholitic condition from north to south, so that the disturbance was felt somewhat later in California, although it was part, doubtless, of the same great subcrustal process.

In the Coast Ranges of California we have much less precise information than in the case of the Sierra Nevada. Analogous conditions seem to be indicated by the information at hand. There are areas of granite and metamorphic rocks which have been subject to great denudation prior to the deposition of the Cretaceous. No rocks of older age than Cretaceous are known to rest upon the worn surface of this complex. Carboniferous fossils have recently been found by Mr. Fairbanks in the Santa Ana Range[1] in a series of rocks into which the granite of the region has been injected. The same geologist informs us of the intrusion of the granite of the Gavilan Range[2] into the Coast Range metamorphics, and of similar relations in the Trinity Mountains in the Northern part of the state.[3] The writer, also, has observed that the granite of the Santa Cruz Range is intrusive in the limestone of the metamorphic complex. Mr. Fairbanks is of the opinion that generally the

[1] Am. Geologist, vol. xi., Feb., 1893.

[2] Loc. cit.

[3] Am. Geologist, March, 1892.

granite of the Coast Ranges is the equivalent of that of the Sierra, but direct evidence of its intrusion into Triassic or Jurassic strata has not yet been adduced. All that can safely be asserted at present, in the opinion of the writer, is that in the Coast Ranges there is a pre-Cretaceous complex of granite and metamorphic rocks analogous to that of the Sierra Nevada; and that there is no evidence yet recorded which is adverse to Mr. Fairbank's correlation of the granites of the two regions.

In Mexico the official map shows conditions which resemble those of the Sierra Nevada. Emerging from beneath the volcanic sheets, or the mantles of Tertiary or Quaternary formations there are, along the western side of the Republic, numerous masses of granite rocks with associated metamorphics. In these metamorphic rocks are occasional patches of Jurassic and Triassic, conservatively limited in the mapping doubtless to the actual areas where fossils have been found to so determine their age. These small patches of known Jurassic and Triassic age are suggestive of the proximate limit in age of the metamorphic series, and yielding to analogy we may be allowed to *suppose* that the granite bears a relation to the Mexican metamorphics similar to that exhibited in the Sierra Nevada of California.

In South America Steinmann[1] calls attention to the important fact of the invasion of the Mesozoic strata of the Cordillera by truly granitic and dioritic rocks. Karsten,[2] also, informs us that in Columbia, Venezuela and Ecuador the Jurassic are the oldest sedimentary rocks, but have been found at only one locality, while the Cretaceous and Tertiary are abundantly developed; and that the underlying basement upon which the Cretaceous rests is largely granitic. Putting Steinmann's and Karsten's information together we seem clearly to have the conditions of British Columbia and California repeated as to the development of a granitic batholite in the Cordilleran belt in pre-

[1] Am. Naturalist, Oct. 1891.

[2] Geologie de l'Ancienne Colombie Bolivarienne, Nouvelle Grenada et Ecuador, par Hermann Karsten, Berlin.

Cretaceous Mesozoic time, followed by continental uplift and great denudation.

From the facts above cited certain conclusions seem to be warranted which may be presented in the form of hypotheses for future examination:

(1) The pre-Cretaceous Mesozoic revolution which has been freely recognized by nearly all Californian geologists was not limited to the western United States, but affected the entire extent of the Cordilleran belt from Alaska to South America.

(2) It is not clear that the revolution was strictly synchronous in all portions of the Cordilleran belt which have been affected. It may have been progressive, and have extended through the time from the close of the Triassic to the close of the Jurassic so as to obliterate the Jurassic seas earlier in some regions than in others.

(3) An essential feature of the revolution was the development of batholitic magmas which invaded the crust, replaced large portions of it, and eventually congealed as plutonic rock of a prevailingly rather acid character.

(4) The development of the batholite, or batholites, was followed or accompanied by continental uplift.

(5) The complex of invading granite and consequent metamorphics is analogous to that of the Archæan and indicates that the conditions which are commonly recognized as Archæan are not peculiar to rocks of that age.

By way of addendum to this brief note it should be remarked that the irruption of granite in South America did not wholly cease with the Mesozoic revolution. Farther south than the countries which have been mentioned, in the Cordillera of the Argentine Republic, Stelzner has shown that this phase of crustal development continued through into the Tertiary. After a narration and discussion of his facts he formulates the following conclusion:

"So mit bleibt denn nur noch die Annahme übrig, dass die als Granite, Syenite und Diorite zu bezeichnenden Andengesteine eruptive Gebilde sind, die theils nach der Jura- und Kreidezeit,

z. Th. sogar erst nach der in der Tertiärzeit erfolgten Ablagerung der buntscheckigen Andesittuffe im gluthflüssigen Zustande emporgestiegen sind und diejenigen Lagerungsverhältnisse eingenommen haben, unter welchen wir sie heute beobachten können."[1]

ANDREW C. LAWSON.

BERKELEY, July 15, 1893.

[1] Beitrage zur Geol. und Palaeont. der Argentinischen Republic, I. Geol. Theil, p. 207.

THE BASIC MASSIVE ROCKS OF THE LAKE SUPERIOR REGION.

III. SKETCH OF THE PRESENT STATE OF KNOWLEDGE CONCERNING THE BASIC MASSIVE ROCKS OF THE LAKE SUPERIOR REGION.[1]

WITHOUT attempting to distinguish critically between the different types of the basic rocks occurring in the Lake Superior region, it will be sufficient for the present to call attention to some of the work done on them, more especially with reference to their microscopical examination. It will not be necessary to refer to all of the articles in which the "traps" of the region have been more or less briefly mentioned, as it will serve our present purpose to allude only to the most important papers on the subject, and to outline, where advisable, the descriptions of the most important rocks as given by various authors. Professor Irving[2] has discussed the theories held by some of the writers with respect to the origin of the traps, but since these, when they differ from the generally accepted theory of an igneous origin for the rocks in question, are found to be opposed to the facts observed, it would be unprofitable to discuss them further. There can be no doubt but that all of the basic, massive rocks found in dykes and beds in the Lake Superior region are truly igneous.

Douglass Houghton[3] first called attention to the wide-spread occurrence of traps around Lake Superior in his Fourth Annual Report as Geologist of Michigan. He identified knobs, dykes and flows of trap, but was unable to distinguish between the numerous varieties of the rock. His observations related principally to the traps in the Archæan and Keweenawan areas in Michigan.

[1] This Journal, Vol. I., p. 433.

[2] The Copper-Bearing Rocks of Lake Superior. Monographs U. S. Geological Survey, Vol. V., p. 7.

[3] Dated 1841. Reprint in Memoir of Douglass Houghton, by Alvah Bradish, Detroit, 1889, pp. 167–168, and 176–182.

Following Houghton, Messrs. Foster and Whitney[1] made an examination of the copper and iron regions of Michigan under the direction of the United States government. In their report on the copper lands, they described briefly the occurrences of dykes and flows of traps in the copper-bearing rocks of the south shore of the lake. Among them they distinguished compact, amygdaloidal, porphyritic, epidotic and brecciated varieties (pp. 69 and 70). In Part II. of the report, in their description of the iron region, they refer to the large dykes in the Animikie rocks on the north shore of the lake (pp. 12–13), and to the dykes of diabase cutting the Archæan schists in the neighborhood of Marquette, Michigan (pp. 18 and 39). They also gave a recapitulation of the characteristics of the traps of the entire region (pp. 85–94), with their chemical and mineralogical composition.

At about the same time that Messrs. Foster and Whitney were engaged in their survey of the copper and iron rocks, Dr. D. D. Owen,[2] with his assistants, was employed in making a geological reconnoissance of the states of Wisconsin, Iowa, and Minnesota. Messrs. D. D. Owen, J. G. Norwood, B. F. Shumard, Col. Whittlesey, and Major R. Owen examined a much larger area than did Messrs. Foster and Whitney, and were therefore not able to give as much detailed description of the rocks observed as the last named geologists succeeded in doing. They, however, mention the occurrence of sheet and dyke gabbros in Wisconsin, and of dyke gabbros in the Animikie of Minnesota.

Following these geologists came many others who examined the Lake Superior region in more or less detail, but added little to the knowledge of the trap rocks of the district, until, in 1871, Professor R. Pumpelly[3] published a paper on "The Paragenesis and Derivation of Copper and its Associates on Lake Superior," in which he described the melaphyres and other basic rocks associated with the copper on Keweenaw Point. After Pumpelly a number of geologists visited the region, but they devoted their

[1] Report on the Geology and Topography of a Portion of the Lake Superior Land District, Part I. Washington, 1850. Part II., Washington, 1851.

[2] Report of a Geological Survey of Wisconsin, Iowa, and Minnesota. By D. D. Owen. Philadelphia, 1852, pp. 142–164, 285, 304–306, 342–417.

[3] Am. Jour. Sci. (3) II., 1871, p. 188.

time principally to the discovery of the relations existing between the several rocks, and made no efforts to divide these into their varieties.

With the establishment of the surveys of Minnesota, Michigan, and Wisconsin, however, an attempt was made to classify with scientific accuracy the basic rocks of these three states. Kloos[1] had already discovered the gabbro of Duluth and had identified a melaphyre from the same place, but had made no very exact determination of either. Among the geologists on the Michigan and Wisconsin surveys, Messrs. Julien, Wright, Wichman, Pumpelly and Irving examined microscopically the rocks of the Huronian and the Keweenawan series of Wisconsin, and of the Archæan, Huronian and Keweenawan of Michigan, and among the descriptions of these rocks which they give may be found very exact accounts of the characteristics of the diabases, gabbros and other basic eruptives of the region.

Messrs. A. A. Julien[2] and C. E. Wright,[3] as early as 1873, mentioned quite fully the greenstones and traps of the Archæan and of the iron-bearing formations in Michigan. The former writer identified many massive and schistose rocks to which he gave the name of diorite, since he found in them hornblende, but no augite. Mr. Wright likewise discovered hornblende rocks which he evidently regarded as original, since he calls them all diorites. Mr. Wright's determinations are the first ones based upon microscopical observations of Lake Superior rocks. Messrs. Brooks[4] and Pumpelly[5] contented themselves with macroscopic examinations of the basic rocks of the iron and copper-bearing series in this state, and in this way distinguished diorites, melaphyres and amygdaloids, while Mr. Marvine[6] divided the

[1] J. H. Kloos: Geologische Notizen aus Minnesota. Zeits. d. deutsch. geol. Gesell. XXIII., 1871, p. 417. Trans. by N. H. Winchell, 10th Ann. Rep. Geol. and Nat. Hist. Survey of Minnesota, for 1881, p. 193.

[2] A. A. Julien: Geological Survey of Michigan, Vol. II., 1873, Appendix A, p. 41.

[3] C. E. Wright: Ib. Appendix C, p. 213–231.

[4] T. B. Brooks: Geological Survey of Michigan, Vol. I., 1873; Part I., Iron-Bearing Rocks, pp. 99–104.

[5] R. Pumpelly: Part II., Copper District, Ib. pp. 7–16.

[6] A. R. Marvine: Part II., Copper District, Ib. pp. 95–116.

rocks of the Eagle River section of Keweenaw Point into greenstone or fine-grained diorites, feldspathic traps or coarse grained diorites, and traps, including the melaphyres and amygdaloids.

Before the publication of the reports of the Wisconsin survey, Messrs. Streng and Kloos[1] communicated the results of their examination of certain Keweenawan rocks occurring in Minnesota and in Wisconsin about the head of Lake Superior. Streng, who did the microscopical work of the investigation, recognized among his specimens melaphyres, augite-diorites, quartz-diorites and a hornblende-gabbro to which reference has already been made in a former article.[2] Pumpelly[3] also had devoted his attention to the rocks of the copper series. He studied more particularly the fine and coarse-grained diabases and melaphyres of Keweenaw Point.

With the publication of Volume III. of the Geological Survey of Wisconsin a more general classification of the Keweenawan rocks of Northern Wisconsin and of Keweenaw Point in Michigan was given by the same author.[4] He distinguished among them diabases, hornblende and orthoclase-gabbros, melaphyres, augite-diorites and porphyrites, the characteristics of which will be mentioned when the discussion of the diabases and gabbros of Keweenawan age is taken up. In the same volume Irving described the rocks of the Huronian of Wisconsin, among which he found gabbros (p. 147), and those of the Keweenawan in the same state (pp. 168 to 193). The hornblende-gabbros and the augite-diorites of Pumpelly he regarded as altered gabbros and diabases, and not as original hornblende rocks. Julien[5] also gave a very excellent account of the microscopic appearance of two olivine-diabases

[1] A. STRENG and J. H. KLOOS: Ueber die Krystallinischen Gesteine von Minnesota in Nord Amerika. Neues Jahrb. f. Min., etc., 1877, pp, 31, 113, 225.

[2] This Journal, Vol. I., p. 447.

[3] R. PUMPELLY: Metasomatic Development of the Copper-Bearing Rocks of Lake Superior. Proc. Am. Acad. of Arts and Sciences, 1878, XIII., Part II., pp. 253–309.

[4] Geology of Wisconsin, III., 1880, p. 29.

[5] A. A. JULIEN: Microscopic Examination of Eleven Rocks from Ashland county Wisconsin. Geol. of Wisconsin, III., 1880, p. 224.

from Ashland county, Wisconsin; and Wichman[1] published a classification of Huronian rocks based on their microscopical examination. Wichman divided the massive basic rocks into diabases, coarse-diabases and diorites. The only other microscopical work done in connection with the Wisconsin Survey is that by the late C. E. Wright, published in the second volume of the reports. In this Mr. Wright[2] mentioned the occurrence of a diorite containing augite in the bed of Black river.

Further, Dr. Wadsworth,[3] in his discussion as to the origin of the jasper and iron-ores of the Marquette region describes briefly the microscopic features of many of the intrusive knobs that are so prominent a feature in the topography of the district. These are declared to consist largely of diabase and coarse basalt, both massive and slightly schistose.

The investigation of the basic rocks of the region had by this time been sufficiently exact, and the number of specimens examined was large enough to give an idea of the characters of the commonest types occurring there, but these investigations had been undertaken by so many different geologists that no exact correlation between the various varieties discovered was possible. No classification of these could be accomplished until some had examined specimens from all the different localities and had compared them with one another. This work was undertaken by Professor Irving[4] in 1881, and was ably accomplished by him in the course of two years. All publications referring to the lithology of the Keweenawan and Huronian formations on both sides of the lake were carefully reviewed, most of the specimens described in them were examined, and the results of this study and examination, together with a great deal of new information gathered

[1] A. WICHMAN: Microscopical Observations of the Iron-bearing (Huronian) Rocks from the Region South of Lake Superior. Ib. p. 600.

[2] CHARLES E. WRIGHT: Geol. of Wisconsin, II., 1878, p. 637.

[3] M. E. WADSWORTH: Notes on the Geology of the Iron and Copper Districts of Lake Superior. Bull. Mus. Comp. Zoölogy, 1881, Vol. VII., p. 36–49.

[4] R. D. IRVING: The Copper-bearing Rocks of Lake Superior. Monograph V., U. S. Geol. Survey, Washington, 1883.

during a trip among the dykes and sheets of the north shore of the lake, were incorporated in a monograph and published under the auspices of the U. S. Geological Survey in 1883.

The greater portion of the volume is concerned with the discussion of the Keweenawan rocks, but a brief synopsis of the character of the Huronian Series is given (pp. 367–409), and in this a few descriptions of Huronian basic eruptives are communicated. A brief synopsis of Irving's results will serve to give an idea of the relations of the different basic rocks to each other, and at the same time will serve as a basis for the present paper.

The original basic rocks of the Keweenawan, according to Irving, embrace gabbros and diabases, an anorthite rock consisting almost exclusively of anorthite, malaphyres and amygdaloids. The rocks described under the various names possess in general the characteristics of the respective types as defined by Rosenbusch in the first edition of his Massige Gesteine. The gabbros are coarse-grained rocks with a dark-gray or black color in the least coarse-grained varieties, and a light-gray color when the plagioclastic ingredient becomes greatly predominant as is apt to be the case in the coarser kinds. Their texture is highly crystalline, and their specific gravity varies between 2.8 and 3.1. The fine-grained basic rocks, whose ordinary type is diabase, make up relatively thin flows, that are almost invariably furnished with vesicular or amygdaloidal upper portions. Externally the diabases are dark in shade, being black, purple, dark green or brown, according as the rock has undergone more or less alteration. In texture they vary from medium fine-grained to cryptocrystalline. The coarser kinds grade into coarse-grained gabbros, but this gradation has never been observed in any one bed. Moreover, the diabases have undergone a great deal more alteration than the coarser gabbros, and are very strongly marked by their external characteristics, both in their fresh and altered states. They therefore seem to Irving to deserve a special name; since they possess the structure of diabases he calls them by this designation. The olivine-free diabases of the ordinary type pass into still finer grained kinds of a black or brown color. Some of these are

entirely aphanitic, and all kinds tend to a porphyritic development, carrying as phenocrysts oligoclase and more rarely labradorite and augite. Like the diabases mentioned above, the diabase-porphyrites are furnished with amygdaloidal upper portions. In the few instances in which the olivine-bearing rocks have an undifferentiated glassy base, they are called melaphyres, although placed among the fine-grained diabases.

The most of the basic rocks of the region are in the form of interbedded sheets. Dykes are rare. When they occur, their material appears to be diabase or diabase-porphyrite. It is rarely coarse enough to be classed with the rocks called gabbro.

In the Huronian areas on the other hand, large dykes of coarse-grained gabbros[1] cut through the sedimentary beds, and with these are intercalated thick beds of gabbro, and occasionally a few thinner ones of diabase.

Since Irving's general classification of the rocks in question a few other publications have appeared in which the petrography of small areas, and the descriptions of hand-specimens are treated.

Messrs. Herrick, Tight and Jones[2] busied themselves during one summer with a study of the rocks around Michipicoten Bay, an arm of Lake Superior extending northeasterly into Canada. Their paper contains but little with respect to the basic eruptives not found in Irving's monograph. Dr. Wadsworth[3] has examined some of the specimens gathered by the Minnesota Survey and has divided the basic rocks into peridotites, basalts, including gabbros, diabases, melaphyres, diorites and norites, peridotites, and rocks regarded as altered andesites. All of Dr. Wadsworth's descriptions are marked by exactness, but the conclusions based upon them are rendered less valuable than they would have been had Wadsworth himself not been compelled to depend upon others

[1] It will be shown later that most of the rocks called gabbro by Irving and others, are not gabbros, but are coarse-grained diabases.

[2] C. L. HERRICK, W. G. TIGHT and H. JONES: Geology and Lithology of Michipicoten Bay. Bull. Scient. Lab. of Denison Univ., Vol. II., Part 2, 1887, p. 120.

[3] Dr. M. E. WADSWORTH: Preliminary Description of the Peridotytes, Gabbros, Diabases and Andesytes of Minnesota. Bull. No. 2, Geol. and Nat. Hist. Survey of Minn., 1887.

for a knowledge of the field relations of the specimens studied. Messrs. Herrick, Clarke and Deming[1] have also studied a few specimens of the gabbro, both ordinary and orthoclastic varieties, from Duluth, but they have added little to what was already known concerning them, except the suggestion of the possible dependence of the orthoclase-bearing varieties upon their environment for the peculiar characteristics which they possess.

The Canadian geologists have likewise been engaged in a study of the rocks on the north side of Lake Superior. Many allusions have been made to the massive sheets and dykes in the Thunder Bay region, but no microscopical descriptions of them have been published, with the exception of a few notes by the present writer appended to a report by Mr. Ingall[2] on Mines and Mining in the Thunder Bay Silver District. In this report the relations of the large dykes and thick beds of diabase or gabbro to the fragmental rocks of the Animikie series north of the lake are carefully sketched, and the microscopic features of the most important rocks are described. In the Appendix,[3] a few altered gabbros and diabases from both sheets and dykes are very briefly characterized. The former of these have the general peculiarities of the gabbro from the great dyke on Pigeon Point, Minnesota, referred to by the writer[4] in an article on certain contact phenomena at this place, and described at greater length[5] in a bulletin of the U. S. Geological Survey. In the first of these two papers, in addition to the reference to the Pigeon Point dyke, a few remarks are made concerning the relations of Irving's orthoclase-gabbros to the more common varie-

[1] C. L. HERRICK, E. S. CLARKE and J. L. DEMING: Some American Norytes and Gabbros. Am. Geol., June, 1888, p. 339.

[2] E. D. INGALL: Report on Mines and Mining on Lake Superior. Geol. and Nat. Hist. Survey of Canada. Montreal, 1888.

[3] W. S. BAYLEY: Notes of Microscopical Examination of Rocks from the Thunder Bay Silver District.

[4] W. S. BAYLEY: A Quartz-Keratophyre from Pigeon Point and Irving's Augite-Syenites. Am. Jour. Sci. XXXVII., 1889, p. 54.

[5] W. S. BAYLEY: The Igneous and other Rocks on Pigeon Point, Minnesota, and their Contact Phenomena. Bull. No. 109, U. S. Geol. Survey, 1893.

ties of the gabbro of the region, but no detailed descriptions of these rocks, nor of the ordinary gabbros, whose modified forms they are supposed to be, are given. Finally, Dr. A. C. Lawson[1] has mentioned some of the characteristics of certain diabases from dykes in the Archæan rocks of the Rainy Lake region, in which the gabbroitic as well as the diabasic structures are well exhibited, the former toward the centers and the latter near the sides of the masses.

The most comprehensive treatment of the "greenstones" and "greenstone schists" of the Lake Superior region is that by Dr. G. H. Williams[2] in his bulletin on the origin of the green schist, supposed to underlie the Huronian in Michigan. In this volume the author not only describes the petrographical features of the schists with which he deals, but he likewise describes in some detail the microscopical characteristics of the diabases, diabase porphyrites, diorites, diorite porphyrites and gabbros, associated with the schists, and from some of which the latter have been derived.

Within the past three years a number of papers have appeared in which reference is made to some of the special features of a few of the coarse basic rocks, both north and south of the lake, but no articles have been published that deal with their general features. Fairbanks[3] has communicated a few notes on the diorites and gabbros in the province east of the north side of Lake Superior. Irving and Van Hise[4] have given a brief synopsis of the characteristics of the diabase dykes and interbedded sheets in the Penokee iron series on the south side

[1] A. C. LAWSON: Notes on Some Diabase Dykes of the Rainy Lake Region. Proc. Can. Inst. for 1887, and Report on the Geology of the Rainy Lake Region. Pt. F., Ann. Rep. Geol. and Nat. Hist. Survey of Can. for 1887-88, pp. 57-73 and 147-164.

[2] G. H. WILLIAMS: The Greenstone Schist Areas of the Menominee and Marquette Region of Michigan. Bull. No. 62. U. S. Geol. Survey, 1890.

[3] H. W. FAIRBANKS: Notes on the Character of the Eruptive Rocks of the Lake Huron Region. Amer Geologist, I. 1890, p. 162.

[4] R. D. IRVING and C. R. VAN HISE: The Penokee Iron-bearing Series of Northern Wisconsin and Michigan. Monograph XIX., U. S. Geol. Survey, 1893. Chap. VII., The Eruptives.

of the lake, and of the gabbro, diabases, diorites, melaphyres and porphyrites of the Keweenawan overlying the Penokee series to the north, while Hall[1] has described a few hand specimens of diabases and gabbros from the Archæan of Central Wisconsin.

Further, in a discussion as to the nature of the diabase sheets interbedded with the Animikie slates and quartzites in Minnesota and Canada, which leads to the conclusion that the former are subsequent intrusions between the clastic beds, Lawson[2] gives a short generalized description of the petrographical characteristics of these rocks, and in a second article[3] he treats of the structure and composition of the anorthite rock of Irving, to which he gives the name anorthosyte. Finally, the writer in two articles refers to the coarse gabbro[4] of north-eastern Minnesota and to the peridotites and pyroxenites[5] associated with it along its northern border.

W. S. BAYLEY.

[1] C. W. HALL: Notes of a Geological Excursion into Central Wisconsin. Bull. Minn. Acad. Nat. Sciences, III., No. 2., p. 251.

[2] A. C. LAWSON: The Laccolitic Sills of the Northwest Coast of Lake Superior. Bull. No. 8, Geol. and Nat. Hist. Survey of Minnesota, p. 30.

[3] A. C. LAWSON: The Anorthosytes of the Minnesota Coast of Lake Superior Ib., p. 2.

[4] W. S. BAYLEY: A Fibrous Intergrowth of Augite and Plagioclase, resembling a Reaction-rim, in a Minnesota Gabbro. Amer. Jour. Science, XLIII. 1892, p. 515.

[5] W. S. BAYLEY: Notes on the Petrography and Geology of the Akeley Lake Region, in North-eastern Minnesota, 1892, p. 193.

A STUDY IN CONSANGUINITY OF ERUPTIVE ROCKS.

WITHOUT being distinctly formulated, the principle of consanguinity recently enunciated by Prof. Iddings has, as a working hypothesis, been the guide of studies made within the last few years on a group of Brazilian eruptive rocks, and the means of arriving at some interesting and, in part, novel results. The method of study followed, partly by plan, partly from force of circumstances, being the comparative study of a group of localities on the assumption of genetic relations between them, rather than detailed work at single points, was similar to what would be applied to the study of a sedimentary group. This method has in this case proved of great advantage, and, as a contribution to the subject of consanguinity, seems worthy of being put on record.

In 1883, the writer, whose previous training had been almost exclusively in the domains of palæontology and the distinctly sedimentary formations, finding himself in a region of crystalline and metamorphic rocks felt the need of acquainting himself with modern petrographic methods. Working in complete isolation without previous instruction in this branch, without material for comparison and almost without literature, he was also without the traditions of the science and preconceived ideas of the relations of the different petrographic groups, and thus free to follow out the lines of investigation suggested by their apparent field relations.

In working over the material at hand in the National Museum at Rio, attention was attracted to specimens of nepheline-syenite, or foyaite (using that term as a general title for the holocrystalline nepheline-orthoclase rocks) and as one of the localities, the peak of Tingua, was readily accessible from Rio an attempt to determine its field relations was resolved upon. This heavily wooded mountain proved a hard nut to crack, and several excursions gave very slender results beyond the fact that

with the predominant foyaite, phonolite and basaltic rocks, which have since been named monchiquites by Prof. Rosenbusch, occurred. These two last types, found only in loose blocks or in small dykes in gneiss that was clearly older than the foyaite, gave no idea of their relations to the latter rock except that at one point a small dyke of phonolite containing polyhedral inclusions of foyaite, like raisins in a pudding, was observed cutting foyaite of the same type as the inclusions. An examination of a series of railroad cuttings between the peak and the city showed a plexus of phonolite and monchiquite dykes together with a peculiar feldspathic rock of syenitic aspect, which, as they did not extend to the city, were suggestive of a possible genetic connection with the eruptive center of Tingua, or of some other similar center in the vicinity.

The occurrence of phonolites, hitherto only known on Brazilian soil on the volcanic island of Fernando de Noronha, suggested a search for phonolitic centers of eruption. About this time a chance collection made by a naval officer from the island of Cabo Frio, 60 miles from Rio, came to hand. As it contained specimens of both phonolite and foyaite, an excursion was resolved upon, guided by the thought that a rocky island on an open coast should give good exposures and thus perhaps prove a better point than Tingua for the study of the problems presented in this mountain. The island, from two to three miles long and from one-fourth to one-half mile wide, was found to give an almost continuous rock exposure about its entire margin. About four-fifths of the island is composed of coarse grained sodalite-bearing foyaite somewhat different from the Tingua type, and like it cut by numerous dykes of phonolite. The remainder consists of augite-syenite of two types, except a small point which is distinctly tuffaceous and cut by innumerable small dykes of a basaltic character. In one place dyke-like masses and large boulder-like inclusions of a pyroxene-plagioclase rock of a gabbro type occur. The coast of the mainland, distant half a mile more or less from the island, is entirely free from rocks of a syenitic character, and is composed of gneiss cut

by numerous dykes of phonolite, monchiquite and augite-syenite porphyry, as well as of diabase which, as it occurs everywhere in the gneiss regions of Brazil, was not taken into account. Although nothing definite on the field relations of these various rocks could be made out, the idea suggested at Tingua of a possible genetic relation between foyaite, phonolite and monchiquite was strengthened by this repetition of the association and mode of occurrence, that is to say, of a central mass of foyaite with apophyses of phonolite and monchiquite. Aside from this, the association of foyaite with augite-syenite, with a plagioclase rock and with tuff of a volcanic character, suggested other lines of investigation not in accord with the usually received notions regarding these rocks.

Before a second projected excursion to Cabo Frio could be realized a chance specimen of foyaite from the Poços de Caldas in southern Minas appeared at the Rio Museum. As a railroad was under construction in this region the idea at once presented itself that, aside from a study of this district, possibly Tingua and Cabo Frio might be studied more advantageously several hundred miles away than at those points themselves. Instead, therefore, of returning to Cabo Frio an excursion was made to Poços de Caldas where the expectations formed were more than realized. About twelve kilometers of almost continuous rock cutting up a steep mountain slope giving one of the finest and most varied exposures of eruptive rocks in the world, was found. Here immense masses of tuff are seen to be cut by both foyaite and phonolite; dykes and sheets of foyaite pass into phonolite at their margins; small masses of phonolite[1] are seen included in foyaite and *vice versa* masses of foyaite are included in phonolite. Considerable masses of a leucite rock, the first known from South America, cut by and buried under phonolite and presenting tuffaceous facies also occur. Small stringers of augite-syenite were noted in the tuffs and phonolite, and nests of

[1] The name phonolite is retained for these rocks since no petrographer, not knowing their association, would ever think of calling them anything else, although some, with that knowledge, prefer to call them nepheline-syenite porphyries or tinguaites.

decomposed crystals, at first taken for analcime, as well as polyhedral inclusions similar to those of the phonolite of Tingua were obtained. To complete the felicity of the excursion a cutting at the foot of the mountain showed the eruptive rocks to be in part, at least, contemporaneous with Carboniferous strata.

With the data here obtained a paper was prepared and presented to the Geological Society of London (Quart. Jour. 43, 1887) announcing the discovery and general distribution of nepheline and leucite rocks in Brazil, and the general conclusion that the Poços de Caldas eruptive center is volcanic in the most restricted sense of the term, that it is of Carboniferous age, and that here foyaite and phonolite occur as different phases of the same magma.[1]

The attack on Tingua was now renewed with the expectation that a diligent search would reveal something analogous to the Caldas region. A trip to the top of the peak showed little of interest beyond a dyke of phonolite cutting foyaite at the very summit. An examination of the margins, well shown by the cuttings of an extensive series of railroad and pipe lines (for the water supply of Rio) at the front, a river valley at the back and roads over the ridge at both ends of the peak, showed that the foyaite is limited to the massif and nowhere presents unequivocally the character of dykes. Two cuttings, one a tunnel, through a spur covered with foyaite boulders as if from the outcropping of a dyke, is conclusive on this point, as only gneiss was found *in situ*. The eruptive rocks are therefore placed like a plaster on the top and slopes of a gneiss ridge in a manner exceedingly suggestive of volcanic conditions. By forcing a way through the dense forest into the crater-like valley of a stream coming from the very heart of the mountain, the long-sought-for evidence of fragmental eruptives and of extensive masses of phonolite in

[1] Subsequent explorations of the Caldas center proves it to be one of the grandest volcanic masses of nepheline rocks known, measuring from fifteen to twenty miles in diameter. Contrary to the first impression the foyaite masses are comparatively insignificant, and the massif is composed essentially of phonolite and tuff with possibly a large proportion of basic leucite rock. A large and important mass of augite-syenite appears to form part of the same volcanic massif.

sheets rather than dykes was found. A complete analogy, as regards the essentially volcanic character of the massif, with the Caldas region was thus established with the addition of evidence of a lava-flow-like character in the foyaite masses. (Quart. Jour. XLVII., 1891).

A chance fracture of a Caldas specimen showing obscurely an appearance of dodecahedral faces on the external surface of the singular polyhedral inclusions so characteristic of the two places, suggested the search for partially decomposed material which by cleaving around the inclusions would show their true form and reveal the mystery of their origin. This search was rewarded with the discovery of free masses of foyaite, like those of Magnet Cove, Ark., having the external form of leucite. The presence of such rock masses with crystalline outlines in both phonolite and foyaite is another link in the chain of evidence of consanguinity of foyaite, phonolite and leucite rocks, while the presence of accessory plagioclase in some of these masses, taken in connection with the occurrence already noted at Cabro Frio, suggests another interesting line of investigation.

Meanwhile another series of studies presented in an unexpected manner certain new and interesting phases of the problem. Work had been commenced on a deposit of magnetic iron ore at Ipanema in the state of São Paulo where, from the extreme decomposition of the rocks and other unfavorable circumstances, but little could at first be made out beyond the association of the ore with a peculiar clay made up in large part of scales of hydrous mica. An ore of similar character at Jacupiranga in the same state was being investigated by Mr. Henry Bauer, a German mining engineer, and the collections sent by him showed the presence at that place of an undescribed type of holocrystalline nepheline-pyroxene rock since denominated jacupirangite,[1] which, by enrichment in iron, passes to an iron ore, and, by secondary alteration of the pyroxene, affords the same peculiar micaceous clay. Certain basic

[1] Am. Jour. of Science, XLI., 1891, p. 311. The same, or a very similar, type was described simultaneously from Finland by Ramsay and Berghell with the name of ijolith (Geologiska Föreningens i Stockholm Förhandlingar, No. 137, 1891).

eruptives in these collections suggested a comparison with the Tingua and Cabo Frio monchiquites, and Mr. Bauer was requested to search for the characteristic rocks of these places, specimens being sent him for comparison. The return mail brought typical specimens of foyaite, and with this indication of a new locality for that rock, and in the hope of being able to study the Ipanema ore deposit more advantageously at another place, an excursion to Jacupiranga was resolved upon. Under the guidance of Mr. Bauer, and aided by subsequent investigations by him and Dr. Eugen Hussak, the district was found to consist essentially of jacupirangite cut by dykes of foyaite with which is associated phonolite, various types of augite-syenite and a micaceous pyroxene-plagioclase rock in such a way that there is no escaping the conclusion of a genetic relation between these various types. Outlying dykes of the plagioclase rock assume in one place the characters of a gabbro, in another, those of a teschenite. Among the outlying dykes of the district are various types of basic eruptives, including leucite-basanite, vosgesite and syenite-porphyry whose relations to the eruptive center are less clear, but which are also suspected to be genetically connected with the nepheline-bearing types. Most interesting is a cryptocrystalline orthoclase-pyroxene rock passing to coarse grained augite-syenite and presenting a tuffaceous facies clearly indicative of volcanic action.

With the clues obtained at Jacupiranga the study of Ipanema became comparatively easy. The jacupirangite type passing to an iron ore was found as a dyke with the facies of a breccia at the margin, traversing decomposed rock which is evidently identical with the compact augite-syenite of Jacupiranga. By diligent search the latter was found in a sound condition and presenting a variety of interesting phases, such as a passage to coarse grained augite-syenite, tuffs identical with those of Jacupiranga and, most interesting of all, a basic facies in which the orthoclase is replaced by phosphate of lime in the form of apatite. A singular mode of occurrence, and one bearing directly on the question of consanguinity, is that of micro and macroscopic inclusions,

or segregations, of both the feldspathic and phosphatic types of augite-syenite in a phonolitic nephelinite, apparently without feldspar. The bulk of the iron ore at this place occurs as rounded nodular segregations associated with apatite in a decomposed rock which was evidently coarse grained and micaceous. This was evidently not jacupirangite, but apparently some peculiar type of nepheline or augite-syenite. Except for the absence of black garnets it apparently corresponds closely with the ore-bearing rock of Magnet Cove, Ark., described by the late Dr. J. F. Williams. It may be noted in this connection that the same character (absence of black garnet) distinguishes the jacupirangite from the ijolith of Ramsay and Berghell.

As in the Caldas region, there is at Ipanema evidence that the eruptive action took place in the late Carboniferous or post-Carboniferous times. This coincidence of age at two of the localities may perhaps justify the assumption (which cannot be directly proven for lack at the other places of sedimentaries intermediate between the very ancient and the very modern), that all of these eruptive centers are substantially contemporaneous. Bearing on this question of age, as also on that of consanguinity, is the fact that in a region characterized by Devonian and probably also Carboniferous strata in Paraguay, Pohlmann has reported nepheline-bearing basalt, and Dr. J. W. Evans has lately communicated specimens of foyaite and augite-syenite from Pão de Assucar on the Paraguay, proving that this mass, hitherto reputed to be granitic, represents another eruptive center similar to those studied in eastern Brazil.

The evidence of consanguinity of foyaite and phonolite consists of an intimate association within limited areas at all of the localities mentioned, except Ipanema, where neither type has as yet been found in a condition to be positively identified; of a direct passage to phonolite at the margins of foyaite masses at Caldas; of inclusions of phonolite in foyaite at the same place and conversely of inclusions, evidently formed *in situ* of foyaite in phonolite at Caldas and Tingua. In this connection may be mentioned an inclusion of the type of

foyaite, described by Prof. Rosenbusch and Dr. G. H. Williams from the phonolite massif of the island of Fernando de Noronha, whose eruption is presumed to be of much later date than that of the continental centers above described. Whatever may be the explanation of the assumption of the leucite form, without the substance of that mineral, by these inclusions at Caldas and Tingua, this phenomenon may also be cited as an evidence of consanguinity. Confirmatory evidence is afforded by the intimate association of typical leucite and nepheline rocks in the Caldas massif, and perhaps also by the occurrence in Paraguay.

The evidence of consanguinity of the augite-syenite type with those bearing nepheline is almost equally complete. At Tingua, where there is an apparent lack of this type, a single large fragment was found as an inclusion in foyaite. At Jacupiranga, a direct passage by disappearance of nepheline, from foyaite to one phase of augite-syenite could be traced, while other phases of the same type were found associated with foyaite in the same dyke or boss. Most interesting is the association of this type at Jacupiranga and Ipanema with nepheline rocks more basic than the foyaites and phonolites, such as the jacupirangites and phonolitic nephelinites, in the latter of which it occurs as an inclusion or segregation. In this connection it is interesting to note the tendency, rare among the orthoclase rocks, of this type to present olivine as an accessory element.

Still more interesting, though less conclusive, are the indications of consanguinity of foyaite with a group of plagioclase rocks hardly, if at all, distinguishable from those of entirely different genetic relations. At Cabo Frio the appearance is certainly that of segregations of a plagioclase rock in the midst of foyaite, though farther investigation is desirable. At Jacupiranga the two types not only occur in the same dyke or boss, but nepheline has actually been observed as a rare accessory in the gabbro-like rock. The appearance of plagioclase in the pseudo-leucite crystals of Tingua bears on the same question, as does also the appearance in a large collection of phonolite from Fernando de Noronha of a single specimen of an andesite-like rock, which unfortunately

was not observed in time to be included in the material sent to Dr. G. H. Williams for study. Apparently there is a group of gabbro and diabase-like rocks whose genetic relations are with the nepheline-bearing rocks rather than with the ordinary members of the groups which they so closely resemble.

The peculiar and varied group of basic dyke rocks recently denominated monchiquites by Prof. Rosenbusch, afford evidences of consanguinity by their almost constant association, as apophyses, with the nepheline-bearing eruptive centers to whose immediate vicinity they appear to be limited. If certain decomposed dykes at Caldas and Ipanema are correctly referred, this group occurs at all the Brazilian localities. A single instance of a basic segregration resembling this type has been observed in a dyke of phonolite. The occurrence within the space of a few meters in the Tingua phonolitic tuffs of three small dykes of this type, of which two, standing alone, would be taken as representing tephrite and limburgite is suggestive of another line of consanguinity. Equally suggestive is the occurrence of vosgesite in the vicinity of the Jacupiranga center of eruption.

Finally the evidence of volcanic action in the presence of fragmental eruptives found at all of the five localities in constant association with types ordinarily regarded as plutonic, such as augite-syenite, is exceedingly suggestive.

ORVILLE A. DERBY.

SÃO PAULO, BRAZIL, Aug. 1, 1893.

THE DISSECTED VOLCANO OF CRANDALL BASIN, WYOMING.*

THE writer in exploring the north-eastern corner of the Yellowstone National Park and the country east of it came upon evidences of a great volcano, which had been eroded in such a manner as to expose the geological structure of its basal portion.

The work was carried on as a part of the survey of this region, under the charge of Mr. Arnold Hague of the U. S. Geological Survey. The paper is an extract from a chapter in the final report on the Yellowstone National Park in process of completion, and the writer is indebted to Major J. W. Powell, Director of the Survey, and to Mr. Hague, chief of the division, for permission to present it at this time in anticipation of the publication of the final report.

The area of volcanic rocks described is but a small portion of the great belt of igneous material that forms the mountains of the Absaroka range, lying along the eastern margin of the Yellowstone Park.

The volcano of Crandall Basin is one of a chain of volcanic centers situated along the northern and eastern border of the Yellowstone Park, which are all distinguished by a greater or less development of radiating dikes, and by a crystalline core eroded to a variable extent.

The Palæozoic and Mesozoic strata, which formed an almost continuous series to the coal-bearing Laramie, had been greatly disturbed and almost completely eroded in places before the volcanic ejectamenta in this vicinity were thrown upon them. The period of their eruption is, therefore, post-Laramie, presumably early Tertiary.

The first eruptions of andesite were followed by those of basalt in great amounts, and these by others of andesite and

*Abstract of a paper read before the British Association for the Advancement of Science, September, 1893.

basalt like the first. This was succeeded by a period of extensive erosion; reducing the country to nearly its present form. Then came the eruption of a vast flood of rhyolite constituting the Park plateau, which was followed in this region by smaller outbreaks of basalt. The last phase of volcanic activity is found in the geysers and fumaroles which have rendered this region famous.

The volcano of Crandall Basin consists chiefly of the first series of basic andesites and basalts. The earlier acidic andesite, which occurs beneath these rocks, appears to be the remnants of eruptions from neighboring centers.

Nothing remains of the original outline of the volcano. The district is now covered by systems of valleys and ridges of mountain peaks that rise from two thousand to five thousand feet above the valley bottoms. The geological structure of the country, however, makes its original character evident.

The outlying portions of the district to the south, west, and north consist of nearly horizontally bedded tuffs, and subaërial breccias of basic andesite and basalt. With these are intercalated some massive lava flows, which are scarce in the lower parts of the breccia, but predominate in the highest parts, above an altitude of ten thousand feet. Here they constitute the summits of the highest peaks.

In contrast to the well-bedded breccias around the margin of the district, the central portion consists of chaotic and orderless accumulations of scoriaceous breccia with some massive flows. These breccias carry larger fragments of rocks and exhibit greater uniformity in petrographical character.

A still more noticeable feature of the central portion of the district is the occurrence of dikes which form prominent walls, and may be traced for long distances across the country. The greater part of them are found to converge toward a center in the highest ridge in the middle of the drainage basin of Crandall creek. A small number converge toward a second center three or four miles east of the first. In the southern part of the district there are many dikes trending toward a center near the

head of Sunlight Basin, about fifteen miles south of the Crandall center.

The center toward which the Crandall dikes converge is a large body of granular gabbro, grading into diorite. It is about a mile wide, and consists of numerous intrusions penetrating one another and extending out into the surrounding breccia, which is highly indurated and metamorphosed in the immediate vicinity of the core. Within the area of indurated breccia the dike rocks become coarse grained rapidly as they approach the gabbro core. This was undoubtedly the central conduit of an ancient volcano, the upper portion of which has been eroded away.

Upon comparing the geological structure of this region with that of an active volcano, like Etna, it is apparent that the lava flows which form the summits of the outlying peaks must have been derived from lateral cones fed by dikes radiating from the central conduit. And assuming that the volcano of Crandall Basin was similar in type to that of Etna, an idea of its original proportions is derived by constructing upon profile sections through the Crandall cone the outline of Etna. If the erosion of the summits of the highest peaks is neglected, the resulting height of the ancient volcano above the limestone floor is estimated at about thirteen thousand four hundred feet. This is undoubtedly too low, and is well within the limits of present active volcanoes. Erosion has removed at least ten thousand feet from the summit of the mountain to the top of the high central ridge in which the granular core is situated, and has cut four thousand feet deeper into the valleys on either side. It has prepared for study a dissected volcano, which, it is hoped, will in time reveal some of the obscurer relationships existing between various phases of igneous rocks.

Petrological Features.—It will not be possible in an abstract to do more than present, in the briefest manner, the more salient features of the petrology of the rocks of this volcano. The rocks are mostly the same as those in various parts of the Yellowstone National Park, some of which have been described in another place. The older acidic breccia consists of fragments

and dust of hornblende-mica-andesite, hornblende-andesite, and hornblende-pyroxene-andesite. They are partly glassy and partly holocrystalline. In some places they appear to pass into the overlying breccia, but in others they have been eroded and weathered before the latter were thrown over them.

The upper breccia, which constitutes the main mass of the volcano, is basaltic as a whole. It consists of pyroxene-andesite and basalt, the latter predominating in the upper part of the accumulation. The massive flows, as far as investigated, are all basalt. The composition varies constantly within narrow limits. A greater part of these rocks contain glassy groundmass.

The rocks constituting the dikes exhibit more variation than the breccias, though the majority of them are like the breccias in composition and habit, being basalt. They are generally more crystalline. A great many dike rocks resemble the basalts in outward appearance, but have little or no olivine, and are more crystalline. The absence of olivine from the more crystalline forms of these rocks appears to be due to the conditions which influenced the crystallization of the rocks and not to their chemical composition. For in some cases what appear in hand specimens to be decomposed olivines are found to be paramorphs after this mineral, consisting of grains of augite, magnetite, and biotite. As the rocks become more crystalline biotite becomes an essential constituent; the porphyritical minerals lose their sharpness of outline and assume some of the microscopical characteristics which they possess in gabbro.

Within the core the coarest grained forms are gabbro. The composition varies in different parts of one continuous rock mass, and also between different intrusions within the core. The transition is from gabbro to diorite with biotite and quartz; and the extreme variety is that form of granite called aplite; the range in silica being from 51.81 to 71.62 per cent.

Fine grained, andesitic equivalents of diorite occur in dikes outside of the core, but none of the most silicious varieties have been found outside of it. From this it appears that toward the end of volcanic activity near the core the composition of the

magmas became more and more silicious, and the volume of the lava erupted smaller. But this change in composition was not uninterrupted, for there are evidences of the alternate eruption of basic magmas as well. Dikes of more silicious rocks are traversed by later dikes of basic rocks. This has taken place both within and outside of the core. Some of these basic rocks are uncommonly low in silica for rocks of this region. They are all found at some distance from the core, with one exception, which is an intrusion within the core. They are lamprophyric in the sense used by Professor Rosenbusch, and approach more or less closely typical camptonites, monchiquites, kersantites, and minettes. They are connected with the basalts of the district by mineralogical and structural transitions.

These exceptionally basic rocks are the chemical complements of the acidic ones in the core and appear to be among the latest extrusions. While they agree with one another in having a low percentage of silica, they differ in the relative abundance of magnesia, lime and iron oxide on the one hand, and of alumina, soda and potash on the other.

As already pointed out by the writer in another place, the variability in composition of all of the volcanic rocks in this volcano illustrates one mode of differentiation of a magma at a particular center of eruption. A comparison of the chemical and mineral composition of the rocks of this district furnishes additinal evidence of the fact that magmas which are chemically similar will crystallize into different groups of minerals according to the conditions through which they pass. Thus chemically similar magmas may form basalt under one set of conditions, and gabbro under others; the first composed of plagioclase, augite, olivine, magnetite and sometimes hypersthene; the second consisting of plagioclase, augite, hypersthene and biotite, besides some magnetite, orthoclase and quartz, with or without hornblende.

Minerals, then, which are primarily functions of the chemical composition of rocks are also functions of the physical conditions affecting crystallization. Some of the conditions under which the molten magmas solidified within the dikes and core of the

volcano of Crandall basin, may be inferred from a consideration of the geological structure of this ancient volcano. The magmas which solidified within that portion of the core now exposed, and those in dikes within a radius of two miles, must have occupied positions at nearly the same distance beneath the surface of the volcano, that is, at a depth of about 10,000 feet and over. The one was as deep-seated or abysmal as the other, and yet their degrees of crystallization range from glassy to coarsely granular.

The influence of pressure on the crystallization is not recognizable either in the size of grain or the phase of crystallization. Marked changes in the crystallization may be traced horizontally in the immediate vicinity of the core. They are rapid near the core, and are accompanied by the induration and metamorphism of the surrounding rocks. They are in a general measure independent of the size of the rock-body, since narrow dikes within the core are coasely crystalline, while much broader ones in the surrounding breccias are very fine grained. It was, unquestionably, the differences in the temperature of the core rocks and of the outlying breccias which affected the degree of crystallization. The former must have been more highly heated than the latter rocks, and the magmas solidifying within them cooled much slower than those injected into the outlying parts of the volcano. In this case the depth at which the magmas solidified appears to have been of little moment in comparison with the temperature of the rocks by which they were surrounded.

The core of gabbro and diorite with an intricate system of veins of middle grained porphyritic rocks, and radiating dikes of aphanitic and glassy lavas, encased in an accumulation of tuffs and breccias with flows of massive lava, constitute an extinct or completed volcano. The central core consists of the magmas that closed the conduit through which many of the eruptions had reached the surface. In solidifying they became coarse grained. The question naturally suggests itself, Are these coarse grained rocks any less volcanic than those that reached the surface? What part of a volcano is non-volcanic?

JOSEPH P. IDDINGS.

NOTES ON THE LEAD AND ZINC DEPOSITS OF THE MISSISSIPPI VALLEY AND THE ORIGIN OF THE ORES.

THE recent closing down of the silver mines of Colorado and other Western states means not only a lessening of the silver production of the country, but also the shutting off of its greatest source of lead supply. During the past few years over two-thirds of the total yield of domestic lead has been from the argentiferous lead ores of Colorado, Utah, Idaho, Montana and Nevada. Unless operations are resumed in the West, the demand must consequently soon be concentrated upon the deposits of non-argentiferous lead in the Mississippi Valley, which have been in the past the sole important producers. A rise in the price of lead is to be expected as a result, which, in turn, will lead to increase in exploitation and development.

The question naturally arises, therefore, to what extent are these Mississippi Valley deposits to be depended upon for future supply. They have been large and almost constant producers in the past; will they continue to be such in the future? The history of their development, which is in many respects remarkable, lends color to the hope that such will be the case, especially in Missouri. Lead mining was begun in that state as much as 170 years ago, and has continued almost uninterruptedly since. Indeed, the first deposit worked, that of Mine La Motte, has up to this year supplied large quantities of ore, the total value of its product to date being in the neighborhood of $8,000,000. The various bodies of ore have shown signs of exhaustion from time to time, and the industry in the state has waned. About the year 1854 the condition was such that even so competent a judge as Prof. J. D. Whitney[1] ventured the prediction that the supply was nearly exhausted, and that the lead mining of Missouri was a thing of the past. But ever after such depression, deeper excavations have developed new bodies of untouched ores, wider explo-

[1] Metallic wealth of the United States, p. 419.

rations have revealed new fields, or improvements in mining and metallurgical methods have made previously rejected ores available. Along with this, the utilization of the associated zinc ores has led to the opening up of deposits which previously lay untouched, enclosing often unexpected quantities of lead. During the past twenty years Missouri's production has reached large proportions. The total amount mined during this period is fully twice that of the preceding 150 years—a startling refutation of the early adverse predictions. The output during recent years has been only second to Colorado's, and this year will probably be first among the states of the Union; the total amount produced to date probably equals, if it does not exceed, that of any other state.

Similar in some respects are the facts of zinc production. The mining of these ores does not, however, date much more than twenty years back, and hence the industry has not suffered much from the vicissitudes of the early mining. The production grew rapidly from its beginning, and now ranks first in the country. The total output up to the present time is nearly equal to the combined total productions to date of all other states in the Union.

The showing for the Upper Mississippi or Wisconsin zinc and lead area is not quite so good. Mining there dates hardly more than 100 years back, and it was not on an active basis before 1823. The period of maximum work was about the year 1845, and soon after this time Prof. Whitney seems to have been of the opinion that its prospects were better than Missouri's, though he predicted a continued decline. The utilization of the zinc ores began about 1860, which tended to sustain the mining industry and the production of lead, though on a much reduced scale. In the early seventies the production of zinc was quite large and something like a resuscitation of mining took place. During the past thirteen years there has, however, been a general decline, and recently little mining has been in progress. At the time of maximum activity, in 1845, the production of lead was about 27,000 tons per annum; but that of zinc ore, in 1872, was only 22,000 tons. The total amount of lead produced to date is prob-

ably something over 650,000 tons, and of zinc ore only about 250,000 tons.

With such facts in mind it is of interest to note that the deposits to which they relate are the subjects of renewed study at the present time, and the prospect of increased demands upon them, above referred to, makes the revival of the discussions of their origin and mode of deposition most timely.

At the recent meeting of the American Institute of Mining Engineers, held as part of the International Engineering Congress, three papers were presented bearing, in whole or in part, upon the ores of the Mississippi Valley, and another, on the Bertha zinc mine of Virginia, described an ore body belonging essentially to the same class. These papers were by Messrs. F. Posepny[1], W. P. Jenney[2], S. F. Emmons[3], W. P. Blake[4], and W. H. Case[5], respectively.

The first of these papers, by Professor Posepny, is a description and discussion of ore deposits in general, in which he advocates their deep-seated origin through the medium of hot solutions derived from great depths. The second paper, by Dr. Jenney, is an exposition of his views concerning the origin of the Mississippi Valley ores, derived from his recent studies in the region. He repudiates the explanation of lateral concentration advocated by Whitney and Chamberlin, and reverts to the old ideas of Owen and Percival, that the ores have come from below, thus harmonizing with Posepny. The other three papers are principally descriptive, though Mr. Emmons quotes Dr. Jenney's conclusions as applied to the Mississippi Valley ores.

Posepny's direct reference to the ores here discussed is brief. He marshals few facts from the region itself in support of his theory, but rather argues, in a negative way, that no great obstacles exist there which would prevent its accceptance. Thus,

[1] The Genesis of Ore Deposits.

[2] The Lead and Zinc Deposits of the Mississippi Valley.

[3] Geological Distribution of the Useful Metals in the United States.

[4] The Mineral Deposits of Southwest Wisconsin.

[5] The Bertha Zinc Mine.

as positive evidence in Missouri, he states that while the deposits away from the granite and porphyry "islands" of southeastern Missouri consist chiefly of lead and zinc ores, "other metals, such as copper, cobalt and nickel occur as the Archean foundation rocks are approached." This circumstance, he states, is "an indication that the source of the lead deposits also is to be sought in depth." Whatever may be the value of this "indication," the facts, as stated, do not hold generally, in the opinion of the writer. Professor Posepny reasons, presumably, from observations made at Mine La Motte, where such conditions exist. At other places, however, these changes in composition are not observed as the crystalline rocks are approached. At Bonne Terre copper pyrite was found in the old *upper* workings containing about four per cent. of nickel and cobalt. It does not characterize the deeper ores. At Doe Run, a mine recently opened, work is prosecuted along the old water-worn pre-Cambrian surface of the Archean granites, amid the very conglomerate boulders, and very little copper pyrite with cobalt and nickel is found. Again, at other localities in St. Genevieve, Franklin, Crawford and other counties, copper ores occur remote from any granite or porphyry outcrops, and well above the basal beds of the Cambrian.

In the way of negative evidence, our author, in considering the Wisconsin deposits, seems to think the absence of ores in the great thicknesses of limestones and sandstones which underlie the productive horizons a by no means conclusive fact as opposed to their deep-seated source, and suggests that the solution may have come up through a passage not yet exposed, and even that fault fissures and eruptive dikes exist which have not been discovered. From the fact that he refers in this connection only to Whitney's report of 1862, we conclude that he has not had access to the later and more exhaustive works of Strong and Chamberlin. Perhaps, with the full light conveyed by these reports and accompanying maps, Professor Posepny might have attached more importance to the objections raised. It is difficult to conceive how such a passage for the solutions as he suggests

could possibly exist without its presence having been revealed and its course traced, with all the widespread mining and exploring which has been conducted in this region during the past seventy years. Neither can one see how the solutions could traverse the intervening great thicknesses of water-soaked sandstone without becoming diffused, in great part at least. The failure to find such a passage and the absence of the ores in the beds assumed to have been traversed, though evidence of a negative character is so strong that it becomes of almost positive value in support of the theory of lateral segregation.

Dr. Jenney, in support of his position, recognizes systems of fault fissures in the ore districts of both south-western and south-eastern Missouri, which cross each other in different directions; these, he considers, served as channels for the metal bearing solutions, and the association of the fissures with the ore bodies he adduces as evidence of such derivation. The deposits of the south-western portion of the state occur almost exclusively in the Mississippian or Lower Carboniferous limestone. Cross fissures or fault fissures in the rocks, if they exist, are not very apparent. The strata are undoubtedly very much shattered in certain limited areas, and have been subjected to extensive subterranean erosion and corrosion and great silicification. Of a system of extensive or considerable faults, recent stratigraphic work in this region has, however, revealed nothing.

In the Cambrian limestones of the eastern part of the state the conditions are somewhat different. Crevices and fissures are there plainly developed, and evidence of considerable faulting is indubitable. In Franklin County such vertical crevices have supplied large quantities of ore. In that portion of the south-east to which reference is especially made, however, and which has produced by far the bulk of the lead, the crevices, whether marking faults or not, are of insignificant dimensions, and the experience has been that they contained themselves little or no ore. On the contrary, the great ore masses consist of galena disseminated through a thickness of the country rock, often of fifty feet or more. At Bonne Terre a tract 1300 feet long by 800

feet wide has been mined out of such diffused ore. The crevices which traverse this ore body are frequently almost blind, and can only be detected by the drip of roof water. These are such as occur in almost any massive rock. Further, one of the most important faults in this region, which traverses the country about two miles north of Mine La Motte, with an apparent throw of 300 feet, is entirely unaccompanied by ore, though the adjacent ground has been prospected with the diamond drill. Again, not a single instance can be recalled by the writer, in those mines which work to the very contact with the underlying granite, where faulting crevices extend down into that rock. They possibly do so extend in some instances, but there is no positive evidence adduceable that they then continue ore bearing. Apart from this, however, the association of ore and crevices, of course, does not denote by any means a deep-seated source for the ore. Such crevices generally act both as channels controlling their distribution, and as receptacles for their accumulation whatever the source of the ores. Hence, a disturbed and creviced region, which is in other respects adapted to the reception of ores, will be their most natural habitat. Therefore the explanation of the localization of the deposits based upon such conditions is equally consistent with any of the common theories of ore derivation. The same, it would seem, can be said concerning the observed paragenesis of the minerals and the growth of crystals, in which Dr. Jenney sees additional foundation for his conclusions. If we accept the broader idea of lateral secretion, which does not demand that a mineral shall be derived from the very rock to which it is attached, but recognizes abundant flow along crevices and through porous strata and a consequent free transfer of solutions from place to place, all the phenomena find at least an equally ready explanation. It is argued further, in this paper, as against the lateral secretion theory, that the metallic contents of the country rocks are insufficient to have supplied the ore bodies. The grounds for this statement are only suggested; but, to the best of our knowledge, the fact yet remains to be proven. Due allowance is not made for the many and various ways in

which minute quantities of substances disseminated through vast volumes of rock may be brought together.

In evidence of the post-Carboniferous age of the deposits the statement occurs several times in Dr. Jenney's paper, that the ores occur in the Coal Measures. This, we think, should be made with limitations. They are found in shales of that age in Jasper county, and at a few other localities, but these shales are in isolated patches, which occupy depressions in the older ore-bearing Mississippian rocks. The metallic contents of the coal may, hence, be derived, by some secondary process of transfer, from adjacent ore bodies. In any case, the Coal Measures in the state, as a whole, are practically destitute of these ores, and they can thus hardly be stated to occur in that formation, whether their absence be due to their prior formation or to limitations in their distribution determined by physical causes.

Dr. Jenney seeks further to find support for the hypothesis of the deep-seated origin of the ores through analogy, in stratigraphy and geologic history, with regions of the far West. This attempt does not seem, in our judgment, to be successful. The last pronounced regional disturbance of both the Ouachita and Ozark uplifts was immediately after the Coal Measure period. In Arkansas this was accompanied by great flexing of the strata. There is no evidence in the Ozark uplift of any intense disturbance of post-Cretaceous date, or of the presence, even at great depths, of flows of such igneous rocks as accompanied the uplift and preceded the ore formation of the Rocky Mountains. As already expressed, the Missouri ores cannot be properly considered to occur in the Coal Measures of the state. Did such a profound fissuring take place in post-Cretaceous times as Dr. Jenney's hypothesis requires, we should expect to find it extending into the body of the Coal Measures, accompanied by the ores. At least faulting or other such exhibition of disturbance would be found, which phenomena do not characterize these rocks.

Over and above these considerations affecting the quality of the support of this theory, there still remain the positive obstacles to be disposed of. The almost entire absence of the

precious metals in the Missouri ores is a fact which further weakens the force of any analogy which may exist between their conditions of deposition and those of the Rocky Mountain ores. How are the objections raised by Whitney and Chamberlin, discussed in a previous paragraph, to be met; such as the facts that faults are practically absent from the region; that there is little ore in the underlying Lower Magnesian beds and none in the Potsdam and St. Peter's sandstones; that no deep and continuous crevices like true fissures are found; that no hydrostatic cause is assigned for the ascension of the solutions from great depths. How could the ores be carried across such thick pervious and water-soaked strata as those of the Potsdam and St. Peter's formations?

The generally accepted facts that the deeper-seated rocks are richer in metallic constituents; that subterranean waters are of high temperature and under great pressure, and consequently are powerful solvents; that the relief of pressure and the diminution of temperature accompanying the ascent of such solutions supply an abundant cause for the deposition of their metallic burdens, are all good and enticing general reasons in favor of the adoption of the theory of a deep source for *all* of our metalliferous deposits. Yet, on the other hand, we must recognize that *some* of our ores, notably those of iron and manganese, cannot be assigned such an origin. Why is it not possible, on general grounds, that other ores should be gathered as are those of these two metals? In reply, it is manifest that we cannot rely entirely upon such general principles, as they are at present understood; but must resort to specific facts in connection with special cases. Few definite facts relating to this Mississippian area have been adduced in these recent papers which can stand as new reasons for believing in the deep origin of the ores, an explanation long since offered by Owen and Percival. Neither have we attempted to introduce positive demonstration in opposition to it. The question seems to be very much *in statu quo*, and, so long as it so remains, the old objections hold good and must be done away with before a change of opinion is warrantable. ARTHUR WINSLOW.

Editorial.

The Lake Superior excursion, under the leadership of Professors Van Hise and Wadsworth, which preceded the scientific meetings at Madison and Chicago, was participated in by a goodly company of foreign and American geologists from whose testimony we learn that it was unusually profitable and enjoyable. It was thoroughly planned, even to minor details, and carried into execution with remarkable precision, no time being wasted by errors or by undue attention to trivial features. Brief lucid explanations by the guides brought out the essential features of the formations and greatly facilitated observation.

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The meeting of the Geological Society of America at Madison was attended by somewhat larger numbers than usually gather at a summer meeting. The following twenty papers were offered and read in full or given in substance, with the exception of two, whose authors were absent, and which were only read by title for lack of time: On the Study of Fossil Plants, by Sir J. Wm. Dawson; On a New Species of *Dinichthys*, On a new *Cladodus* from the Cleveland Shale, and On a Remarkable Fossil Jaw from the Cleveland Shale, by E. W. Claypole; Origin of Pennsylvania Anthracite, by J. J. Stevenson; The Magnesian Series of the North-western States, by C. W. Hall and F. W. Sardeson; On the Succession in the Marquette Iron District of Michigan, by C. R. Van Hise; Extra-morainic Drift in New Jersey, by G. Frederick Wright; On the Limits of the Glaciated Area in New Jersey, by A. A. Wright; South Mountain Glaciation, by Edward H. Williams, Jr.; Terrestrial Subsidence South-east of the American Continent, by J. W. Spencer; Evidences of the Derivation of Kames, Eskers, and Moraines of the North American Ice-sheet, chiefly from its Englacial Drift, and The Succession of Pleistocene Formations in the Mississippi and Nelson River Basins, by Warren Upham; The Cenozoic History of Eastern Vir-

ginia and Maryland, by N. H. Darton; Notes on the Geological Exhibits of the World's Fair, by G. H. Williams; Dislocation of the Strata of the Lead and Zinc Region of Wisconsin and their Relation to the Mineral Deposits, with some observations upon the Origin of the Ores, by W. P. Blake; Geology of the Sandhill Region in the Carolinas, by J. A. Holmes; The Gravels of the Glacier Bay in Alaska, by H. F. Reid; The Arkansas Coal Measures in their Relation to the Pacific Carboniferous Province, by James Perrin Smith; Glaciation of the White Mountains, N. H., by C. H. Hitchcock.

Professor Reid's paper on the Gravels of Glacier Bay was given the form of an illustrated evening lecture, and was found entertaining and instructive by the popular audience as well as the members of the society. By admirable photographic illustrations he brought forth very clearly and impressively many of the features of glacial action. It was peculiarly valuable as illustrating the behavior of alpine glaciers when they reach unusual magnitude, and particularly when they approach the Piedmont type.

The paper of Sir J. Wm. Dawson does not admit of ready synopsis. It needs to be read in full. Professor Claypole presented a number of interesting and apparently important facts relative to fossil fishes from north-eastern Ohio.

One of the more notable papers was that of Professor Stevenson, in which objections were urged against the current doctrine of the origin of anthracite through metamorphic agencies connected with heat and pressure. In lieu of this hypothesis, which the author held to be untenable, an hypothesis was offered con necting the origin of anthracite with the conditions of deposition. Anything less than a full statement of the author's view in his own language would fail to do it justice.

The paper of Professor Hall and Mr. Sardeson, read by the latter, endeavored to correlate, in much detail, the series of magnesian limestones of the north-western states. The most notable feature was the placing of the dividing horizon between the middle and the upper Cambrian considerably higher than has been done by most previous writers, throwing the larger part of the

light-colored sandstones that lie below the alternating series into the middle rather than the upper division.

Professor Van Hise gave a lucid sketch of the succession of deposits in the Marquette district and the grounds on which his interpretation is based. The paper showed the steady progress that is being made in the disentanglement of the gnarled structure of that region.

The papers of the Professors Wright awakened special interest from their relation to previously controverted ground. Contrary to their recent contention, they now extend the glaciated area so as to include the localities of High Bridge and Pattenburg and a considerable territory in the Triasic region essentially as maintained by Professor Salisbury before the Professors Wright took up the special study of the matter, though this was not as distinctly acknowledged as might have been desired. The discussion on the part of Chamberlin and McGee took the congratulatory form in view of the removal of one important point of difference and the advance toward harmonious views. It was noted that the points of difference were essentially reduced to two: The correlation of the Trenton gravels and the age of the extra-morainic drift relative to the moraine. In regard to this last it was pointed out that an important contribution had been made, unwittingly perhaps, to the presumption of great difference in the ages of the two drifts, in the fact that the outer drift, especially at such localities as High Bridge and Pattenburg, where it is thick, could not be presumed to be of the same age and character as that of the moraine and moraine-bordered drift, or its glacial origin would not have been previously denied by the Messrs. Wright, and that its age must be presumed to be very much greater or it could not have been referred to a residuary origin, especially to residuary derivation from formations which have disappeared from the neighborhood, since the moraine and moraine-bordered till are very distinctly characterized glacial formations of fresh aspect, while residuary accumulations and residuary topography are inherently expressions of age.

Dr. Spencer submitted a large mass of valuable data relative

to submerged channels in the south-eastern part of the continent, particularly the Antillean region, and urged these as evidences of very great subsidence. The paper awakened considerable discussion, the general tenor of which was the acceptance of the evidence and of the inference of subsidence, with an expression of doubt as to the time of its occurrence and its relations to other geological events.

The paper of Mr. Upham was a fuller statement of the arguments he has recently advanced in support of the derivation of kames, eskers, and moraines chiefly from englacial drift. These, and his views of the internal movement of the ice upon which they are in some degree founded, were opposed by Reid on physical grounds and by others on observational grounds. It was remarked that existing glaciers fail to show basally-rubbed material on their surfaces, even on their low terminal slopes, at least as a common fact. In his second paper, Mr. Upham urged a somewhat simple and brief succession of Pleistocene formations. The successive lines of moraines and the observed overlaps of till were interpreted as signifying minor and relatively brief halts and readvances of the ice. In the discussion, this position was opposed as being inconsonant with the evidences of interglacial intervals and of intervening erosions, oxidations and other changes which the formations were thought to present.

The papers of Darton and Holmes on different but analogous portions of the coastal region showed the very great advances which have been made in the last few years in the analysis and differentiation of the coastal formations, and the interesting discussions they called forth showed, in some measure, the important bearing these have upon the interpretation of the Pleistocene and immediately Pre-Pleistocene histories of the glaciated region.

Professor W. P. Blake, while coinciding in general in the views held by Whitney and by Chamberlin respecting lead and zinc deposits, urged the existence of a greater amount of dislocation than they had recognized, and attributed to it greater influence in the localization of the deposits. His views are intermediate

between those of the authors mentioned and those recently advanced by Mr. Jenney.

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THE attendance upon the meeting of the American Association was less than usual, but the interest and the character of the papers compared favorably with those of other sessions. The provisions made by the local committee were excellent, and the hospitalities extended by the citizens of Madison were graceful and generous. The exceptional beauties of the place and the superb weather lent attractiveness to the occasion.

In the Geological Section, the following papers were offered, and, with few exceptions, read in full or in substance: Gravels of Glacier Bay, Alaska, with lantern illustrations, by H. F. Reid; Use of the Name "Catskill," by John J. Stevenson; Section across the Coastal Plain Region in Southern North Carolina, by J. A. Holmes; Notes on Further Observations of Temperature in the Deep Well at Wheeling, W. Va., by William Hallock; Recent Investigations in the Cretaceous Formation on Long Island, N. Y., by Arthur Hollick; Character of Folds in the Marquette Iron District, by C. R. Van Hise; The Fossil Sharks of Ohio, by E. W. Claypole; Hillsdale County Geology, by Horatio P. Parmelee; Exhibition of Trilobites, showing Antennæ and Legs, by Chas. D. Walcott; Remarks on the genus *arthrophycus* Hall, On the Value of Pseudo-algæ as Geological Guides, Studies in Problematic Organisms, and The Genus *Fucoides*, by Joseph F. James; Northward Extension of the Yellow Gravel in New Jersey, Staten Island, Long Island and Eastward, by Arthur Hollick; Some Questions Respecting Glacial Phenomena about Madison, by T. C. Chamberlin; Amount of Glacial Erosion in the Finger Lake Region of New York, by D. F. Lincoln; Ice-sheet on Newtonville Sandplain, by F. P. Gulliver; Additional Facts Bearing on the Question of the Unity of the Glacial Period, by G. Frederick Wright; Changes of Drainage in Rock River Basin in Illinois, by Frank Leverett; Graphic Comparison of post-Columbia and post-Lafayette Erosion, by W J McGee; An Illustration of the Effect of Stagnant Ice in Sussex Co., N. J., and A Phase of Superficial Drift, by R. D. Salisbury;

Tertiary and Quarternary Stream Erosion of North America, by Warren Upham; The Emergence of Springs, by T. C. Hopkins.

As the writer was unable to hear a considerable number of these papers his notes must be confined to comparatively few of them. The paper of Mr. Lincoln presented a very interesting sketch of the quite remarkable evidences of glacial erosion and modification of surface in the Finger Lake region of New York. He showed, successfully we think, that the existing topography could not have arisen in its present form through the agency of subaërial degradation alone nor by the simple deposit of drift material on a surface so produced, but that a very notable amount of reshaping of the rock-surface was the result of glacial abrasion.

Mr. Frank Leverett made a quite important contribution to the data bearing upon the stages and duration of the earlier glacial epoch. He has recently discovered evidence that the Rock River formerly flowed nearly due south from a point near Rockford into the Green River basin, and presumably onward to the great bend of the Illinois River, near Hennepin, where an old deep channel exists. From this course the river was diverted to its present south-westerly course by the earliest or at least one of the earlier stages of the ice invasion of that region. Between the time of this diversion and the stage at which the kettle moraine was formed across the Rock River about forty miles to the north, near Janesville, Wis., the river cut a trench in rock across a succession of preglacial cols to maximum depths estimated at 100 to 125 feet. Mr. Leverett made careful estimates of the total amount of rock excavation and found it to amount to one square mile 1100 feet deep. Stated in another form, this equals a trench 100 feet deep, one mile wide and eleven miles long, or one-half mile wide and twenty-two miles long. After the trench had been cut, the glacial wash from the outer edge of the kettle moraine partially filled the trench as shown by remnants of terraces still existing at different points along it. The amount of this filling within the area of the above computation is estimated as one square mile 900 feet thick or $\frac{9}{11}$ of the amount of rock excavation. Since the formation of these gravels the

stream has only partially removed this partial filling of the trench previously cut. The estimated amount of the material so removed since the time of the formation of the kettle moraine is one square mile 650 feet deep, or $\frac{1}{2}\frac{3}{5}$ as much as the *rock* excavation. From this it appeared that the amount of erosion in all post-glacial time (including the last of the glacial period), although wrought upon incoherent gravels, is much less than the amount of rock cutting accomplished between the time the river was diverted and the formation of the kettle moraine.

In the introduction to his paper Professor G. Frederick Wright stated that the hypothesis of an ice dam at Cincinnati appeared to be in a damaged condition, as an agency to account for the high terraces of the upper Ohio and some of its tributaries, and that it was a part of the purpose of the paper to repair the damage. It proved in the sequel, however, an effort at emendation by substitution. The additional facts bearing upon the unity of the glacial period cited in the paper related chiefly to a considerable depth of glacial wash in the trench of a tributary of the Beaver River near Homewood, Pa., just outside but near the border of the glaciated region. Professor Wright contended that the trough in which this glacial material lies must have been eroded previous to its deposition. This erosion he referred to pre-glacial times. The filling reaches nearly or quite to the upper terrace plain on the north side of the tributary, but does not appear on the terrace plain south of the tributary. In the course of his paper, and notably in the discussion following, Professor Wright advanced the hypothesis that the rock shelves which constitute the base of the high terraces of the upper Ohio, Allegheny and adjacent rivers, were formed during a stage of base-levelling in Tertiary times, that the narrower and deeper valley below the rock shelves (in round numbers 300 feet deep) was cut in this base-plane during a stage of elevation just preceding the glacial period, and that this trench was filled up with glacial wash and glacio-natant material to a height, at some points, as much as sixty feet above the rock shelves. In the discussion it was pointed out that, to

account for the fact that the trains of gravel that rise on the outer face of the adjacent moraines run down through this narrower deeper valley at low levels, it is necessary to suppose that there was an interruption of glacial action and a period of excavation during which the previously formed 300 feet or more of glacial wash was very largely carried away, and that this means a discontinuity of glacial action and an interglacial interval. The hypothesis is, therefore, not a contribution to unity but to discontinuity. The amount of excavation between the time of the supposed first filling of the trench and the partial refilling at the time of the formation of the adjacent terminal moraine was several times greater than all that has taken place since the moraine was formed. It signifies, therefore, a very notable interruption of continuity and a reversal of action. It may be here added that, logically, it also means the abandonment of the "fringe" theory to account for the older drift, for the filling of the valleys for so great distance and to so great depth means more than a trivial stage of advance, and the excavation previous to the formation of the moraine means more than a slight stage of recession.

Mr. Leverett has examined the Homewood locality since the meeting, and became satisfied that the partial filling of the trench at that point took place contemporaneously with a moraine which crossed the valley only a short distance above (some miles outside the glacial boundary as mapped by Lewis and Wright, and even some distance beyond the striæ not long since reported by Dr. Forshay, Mr. Leverett finding striation half a mile farther down the valley). The characteristics of this moraine seem to Mr. Leverett to indicate that it belongs to the group formed during the later incursion. The shelf of rock south of the tributary was not covered by the glacial wash of this stage because the trench lacked about twenty feet of being filled by the wash. Mr. Leverett found other remnants which he regards as parts of the same glacial flood-deposit farther down the Beaver, the surface rapidly descending as is the habit of such moraine-headed terraces near their sources. The facts

here, therefore, appear to be essentially the same as on other tributaries of the region which are crossed by the group of later moraines, and which seem to indicate profound excavation between the earlier and later drifts.

The hypothesis advanced in the paper, while not new in itself, having been among the multiple working hypotheses used by one or more students of the region, though not so far as known adopted by any one previously, is much more deserving of serious consideration than its predecessor, the Cincinnati ice dam. It may have some elements of truth in it, *i. e.*, a portion of the excavation of the rock below the old base-plane may have preceded the incursion of the glacial wash and even the glacial period. If this should prove true the effect will be to extend the importance of the earlier glacial epoch and to reduce the time necessarily attributed to the interglacial interval of excavation. The glacial formations of the lower Ohio and adjacent regions, however, seem to indicate a more complex hypothesis than this, or any previously advanced, which shall take cognizance of more than one glacial episode previous to the formation of the well-developed terminal moraines.

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One session of the Geological Section was adjourned to permit members to listen to papers read before the Anthropological Section having a geological bearing. These were the "Evidence of Glacial Man in America," by G. Frederick Wright; and "The Antiquity of Man in America," by W J McGee. The former consisted essentially of a restatement of the supposed evidences of the existence of man contemporaneously with the glacial period found in the terraces at Madisonville and Newcomerstown in Ohio, and at Trenton, N. J. The latter consisted essentially of a discussion of the character of evidence required for the establishment of the antiquity of man. Emphasis was especially laid upon the distinction between legal evidence and scientific evidence.

In the first paper no new discoveries were announced nor any additional data of note added to previous evidence. On

the other hand, the localities of Little Falls, Minn., Medora, Ind., and Loveland, Ohio, which have recently been urged as offering evidence of glacial man, were passed in silence. The paper referred constantly to the chipped stones as "paleolithic implements," and ignored the recent issue raised by Professor Holmes' investigations which are thought by many to make it probable that, whatever their geological age, the chipped stones are rejects and failures incident to the process of neolithic manfacture, and are therefore neither "paleolithic" nor "implements" in the proper sense of the terms. In the discussion, attention was called to the significant omission of three out of six of the localities which a year ago were urged as furnishing evidence of glacial man. Attention was called to the Ohio exhibit in the Anthropological Department of the Exposition in Chicago as furnishing proof that the testimony relating to the Newcomerstown locality cannot be accepted as having scientific value, because the point marked upon the photographs of the exhibit as being the location of the find cannot be rationally supposed to be the actual locality. Considerable discussion also turned upon the possibilities of intrusion, particularly through the agency of the growth and decay of the roots of successive generations of forests. It was urged that, allowing not more than six thousand years since the close of the glacial period, and allowing one hundred years for a generation of trees, sixty generations may have grown in succession. In the process of the growth of the large roots of the trees, the gravels and other material were pressed laterally and to some extent upward by their expansion, and on the decay of the roots the space they occupied was refilled, presumably from above, in part at least. In the case of trees which have tap roots the penetration is deep, particularly on gravel terraces where the substratum is porous and relatively dry and the ground-water far below the surface. It was urged that, in the refilling of the numerous tubes formed by the growth and decay of the roots of so many generations of trees, opportunities would be afforded for the occasional and sometimes deep penetration of relics that were originally

deposited at or near the surface. It was objected that the tubes formed by roots would be closed in by lateral creep and not from above. This, it may be here remarked, would depend upon whether the lower part of the root decayed before the upper part, or whether the decay proceeded from the surface downward. It would also depend upon whether the exterior of the roots rotted first or whether the bark resisted decay longest, leaving the interior, at a certain stage, practically hollow. It would appear that this subject has not received adequate attention, and that careful investigations respecting the growth and decay of roots in such situations should be made, and the possibilities of intrusion by means of them carefully determined. Reference was also made to the possibilities of intrusion through the agency of a similar succession of generations of burrowing animals. In view of the fact that in the paper under discussion only about twenty flaked stones of artificial origin were insisted upon as occurring deep within the gravels, the question of the possibilities of intrusion assumes very considerable importance. A certain amount of intrusion can fairly be claimed as probable. The vital question is, Can it be presumed to account for all cases not otherwise accounted for?

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THE admirable address of the retiring President of the American Association, Dr. LeConte, appears in this number of the JOURNAL and needs no comment. We hope to publish Vice-President Walcott's address in our next number.

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THE Woman's Section of the Geological Congress at Chicago, assembled on Monday, August 21, and held short sessions throughout the week. The following is the list of papers:

Methods of Teaching Geology, by Miss Mary Holmes, Ph.D., Rockford, Ill.; Physical Geology, by Miss Mary K. Andrews, Belfast, Ireland; Chemical Geology, by Miss Louise Foster, Boston, Mass.; Granites of Massachusetts and Their Origin, by Mrs. Ella F. Boyd, Hyde Park, Mass.; Artistic Geology, by

Mrs. S. Maxon-Cobb, Boulder, Colo.; The Geology of Ogle County, by Mrs. C. M. Winston, Chicago; The Fossils of the Upper Silurian, by Mrs. Ada D. Davidson, Oberlin, Ohio; Crinoidea and Blastoidea of the Kinderhook Group as found in the Quarries near Marshalltown, Iowa, by Jennie McGowen, A.M., M.D., Davenport, Iowa; The Evolution of the Brachiopoda, by Miss Agnes Crane, Brighton, England; The Mastodon in Northern Ohio; Post-Glacial or Pre-Glacial? by Miss Ellen Smith, Painesville, Ohio; Palæontology, by Miss Jane Donald, Carlisle, England; Glacial Markings, by Miss Thomson, Newcastle, England.

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THE general session of the Geological Congress convened at Chicago on August 24, immediately following the close of the meeting of the American Association at Madison.

The Congress was welcomed felicitously by the President of the Auxiliary, Charles C. Bonney, and briefly by the Chairman of the Committee on Organization.

Dr. A. R. C. Selwyn presided over the first session; Professor Joseph LeConte and Mr. Hjalmar Lundbohm, of Sweden, over the second session; and Professor James Hall and Dr. Groth, of Munich, over the third. The following papers were presented:

Pre-Cambrian Rocks of Wales, Dr. Henry Hicks, London, England; The Classification of the Rock Formations of Canada, with Special Reference to the Paleozoic Era, by Henry M. Ami, Geological Survey of Canada; The Cordilleran Mesozoic Revolution by Dr. A. C. Lawson, University of California; The Oil Shales of the Scottish Carboniferous System, by Henry M. Cadell, late of the Geological Survey of Scotland; Distribution of Pre-Cambrian Volcanic Rocks along the Eastern Border of the United States and Canada, by Professor George H. Williams, Johns Hopkins University; Huronian versus Algonkian, by Dr. A. R. C. Selwyn, Director Geological Survey of Canada; On the Migration of Material during the Metamorphism of Rock Masses, by Alfred Harker, St. John's College, Cambridge, Eng-

land; Wave-like Progress of an Epeirogenic Uplift, by Warren Upham, Geological Survey of Minnesota; Zur Nereiten Frage, by Dr. H. B. Geinitz, Dresden; Genetic Classification of Geology, by W J McGee, Bureau of Ethnology; The Extent and Lapse of Time Represented by Unconformities, by Professor C. R. Van Hise, U. S. Geological Survey; Restoration of Clidastes (illustrated), by Professor S. W. Williston, University of Kansas; Glacial Succession in the British Isles and Northern Europe, by Dr. James Geikie, Geological Survey of Scotland; Glacial Succession in Sweden, by Hjalmar Lundbohm, Geological Survey of Sweden; Glacial Succession in Switzerland, by Dr. Albrecht Heim, Zurich; Glacial Succession in Norway, by Dr. Andr M. Hansen, Geological Survey of Norway; The Succession of the Glacial Deposits of Canada, by Dr. Robert Bell, Canadian Geological Survey; Glacial Succession in the United States, by Dr. T. C. Chamberlin, University of Chicago; Pleistocene Climatic Changes, by Warren Upham, Geological Survey of Minnesota; Evidences of the Diversity of the Older Drift in North-western Illinois, by Frank Leverett, U. S. Geological Survey. A paper on the General Geology of Venezuela, by Dr. Adolph Ernst, was omitted on account of the illness of its author; and two papers by Dr. O. A. Derby, entitled, On the General Geology of Brazil, and On the Eruptive Phenomena of Brazil, were omitted because their author did not arrive until after the session. Four other papers announced were not read.

The latter part of the first session was devoted to a general discussion of the question, *Are there any Natural Geological Divisions of World-wide Extent?* The latter part of the second session was devoted to the question, *What are the Principles and Criteria to be observed in the Restoration of Ancient Geographic Outlines?* The general question assigned for the third discussion, *What are the Principles and Criteria to be observed in the Correlation of Glacial Formations in Opposite Hemispheres?* was omitted to give time for the discussion of the preceding glacial papers.

Several of the papers read will appear in this JOURNAL, and some of the matters touched upon in the discussions may be the

subjects of subsequent comment. About one hundred geologists were in attendance, a number which, under all the circumstances, was greater than was anticipated.

The afternoons of each day were devoted to the Exposition. Superintendent F. J. V. Skiff, Chief of the Department of Mines and Mining, and his associates, gave the members of the Congress a very pleasant welcome on their initial visit and provided special privileges of inspection that were heartily appreciated.

T. C. C.

Reviews.

Eruptive Rocks from Montana. By Waldemar Lindgren. Proc. Cal. Acad. Sci. Ser. 2, Vol. 3. 1890.

A Sodalite-Syenite and other Rocks from Montana. By W. Lindgren, with analyses by W. H. Melville. Am. Jour. Sci. Vol. 45. April 1893.

Acmite-Trachyte from the Crazy Mountains, Montana. By J. E. Wolff and R. S. Tarr, Bull. Mus. Camp. Zoölogy, Harvard College. Vol. 16, No. 12. (Geological Series, Vol. 2).

Contributions to our knowledge of the mineral and chemical composition as well as the relationships of the igneous rocks of particular regions, however fragmentary, are of the greatest importance; especially when they relate to the vast areas of North America which remain almost unknown to the petrologist. The exploration of the great belt of country, one hundred miles wide, extending from California to Colorado and Wyoming along the fortieth parallel of latitude, by the geologists under Mr. Clarence King, constitutes the one great systematic study of the volcanic rocks of any considerable area on this continent. Less extensive investigations of smaller areas, isolated from one another and often separated by long distances, have been made from time to time, and to some extent have been published. But a large part of the work already done has not yet been printed. The facts so far brought to light show that the rocks of the Great Basin and the Pacific coast differ as a whole from those occurring in the eastern portion of the Rocky mountains and the region immediately east of it. This difference consists mainly in the greater abundance of the alkali-bearing rock-making minerals in the rocks of the latter region, caused by the relatively higher percentage of sodium or potassium in the magmas from which they have been derived.[1]

The recent papers by Mr. Lindgren and by Messrs. Wolff and Tarr illustrate this characteristic of the volcanic rocks of Montana along the frontal ranges of the Rocky mountains. All of the rocks described occur as intrusive bodies; laccolites, sheets, dikes or necks. They

[1] J. P. Iddings: The Origin of Igneous Rocks. Bull. Phil. Soc. Washington, Vol. 12, pp. 138, 139, 184.

were erupted in early Tertiary or late Cretaceous time in most cases, but their exact date is not known. Owing to extensive erosion the extrusive forms of these rocks, if they ever reached the surface, have been entirely removed.

Mr. Lindgren observes in the first paper cited that the rocks of this region appear to be more varied in chemical composition than the series usually found in the Great Basin; magmas rich in potassium are frequent, crystallizing as trachytes; often they are very basic, and contain much sodium, resulting in the abundant separation of such minerals as nepheline, sodalite and analcite.

The more or less acid rocks in the Little Belt mountains and at various points in front of the main range, west of Fort Benton, constitute dacites, hornblende-andesites, and diorites. Similar rocks also occur in the Moccasin mountains. They vary much in structure and composition, and form a natural group. The prevalent habit is porphyritic, but there appears to be a continuous series of transitions from porphyritic to fine granular rocks. The phenocrysts are feldspar and hornblende, and sometimes quartz and mica. The porphyritical feldspars are in part orthoclase in varying quantities, and there is reason to believe that these rocks pass by gradual transitions into trachytic and rhyolitic forms.

Those varieties free from phenocrysts of orthoclase and quartz grade into medium grained diorite, analogous to Stelzner's "Andendiorit," which contain besides plagioclase, hornblende and biotite, a little orthoclase and quartz as the last minerals to crystallize.

Of the more basic rocks, a part are syenites and trachytes, and a part basalts. The syenites which form dikes consist principally of orthoclase, plagioclase, biotite and a pyroxene, probably malacolite. They are called augite-syenites. The syenite from near Dry Fork, Little Belt mountains, contains, in addition to these minerals, allotriomorphic grains of an isotropic substance, probably sodalite. The rock contains 5.50 per cent. of K_2O, and 4.14 per cent. of Na_2O. The augite-syenite from the Highwood mountains is coarsely granular, and contains 5.66 per cent. of K_2O and 7.88 per cent. of Na_2O. This syenite is surrounded by trachytic and basaltic dikes; and in one case a dike of syenite was seen cutting one of the basaltic dikes.

The syenite from Square Butte at the northern end of the Highwood mountain is characterized by a noticeable percentage of sodalite and analcite, and has been called sodalite-syenite. Its chief constit-

uents are orthoclase, albite and hornblende. The relative proportions of the minerals has been estimated to be: orthoclase, 0.50; albite, 0.16; hornblende, 0.23; sodalite, 0.08; analcite, 0.03. The hornblende was analyzed and found to correspond to barkevikite. Mr. Lindgren calls attention to the resemblance in chemical composition between this rock and many nepheline-syenites, except for the relatively higher percentage of K_2O in the rock from Square Butte. He also notices the striking similarity between the analysis of this rock and those of certain leucitophyres from Rocca Monfina, and remarks that under different conditions the same magma, now crystallizing as a sodalite-syenite, might have produced a leucite-feldspar rock.

Trachytic rocks, with a great variety of habits, are abundant in the Highwood mountains. The essential minerals are sanidine and augite, with less prominent biotite. The augite is deep green, often somewhat pleochroic, and evidently contains an admixture of the ægirine molecule. It is very characteristic not only of the trachytes but also of the basaltic dike rocks of this region. These rocks form a connected series, the members of which differ in the relative quantities of augite and sanidine composing them. At one end of the series is a rock consisting almost wholly of feldspar, and at the other end a dark basaltic rock with porphyritical augites and a groundmass of sanidine and augite. In structure these rocks range from holocrystalline and granular to glassy. Some of the trachytes contain small crystals of sodalite (?) inclosed in sanidine. In one form of the rocks sanidine ceases to be the prominent phenocrysts and augite takes its place, and olivine occurs in the groundmass, which consists of feldspar and colorless glass easily soluble in HCl. Associated with the sodalite-syenite of Square Butte are dark colored basaltic rocks, which occur in three sheets at the base of the butte. Surrounding the butte there are numerous dikes apparently radiating from the central mass. One of these basaltic sheets contains phenocrysts of augite, olivine, brown mica, and white isometric crystals whose original character is uncertain. The rock is considerably decomposed. Another of the sheets is like analcite-basalt but is also decomposed. The third is coarsely granular and approaches theralite in composition.

The rocks described as analcite-basalts occur in dikes and possibly as necks in association with the rocks already described. They consist of augite, olivine, magnetite, and a mineral, which from its form and optical properties, and from its chemical composition appears to

be analcite. Biotite is sometimes present in small quantities. From the very fresh appearance of these rocks it seems probable that the analcite is a primary crystallization from the molten magma. The groundmass of the rock consists of augite and small crystals of analcite with magnetite. Mr. Lindgren calls attention to the difficulty of distinguishing glass, if present, from isotropic analcite.

In the Bear Paw mountains there are dikes of rocks related to those just described and which correspond to the lamprophyres of Rosenbusch. They are dark, fine grained, and porphyritic with phenocrysts of augite and long flakes of brown mica. The groundmass consists mostly of lath-shaped plagioclase, augite and mica. Some varieties with phenocrysts of olivine and augite, in a glassy groundmass without feldspar, approach certain limburgites.

The paper by Messrs. Wolff and Tarr is confined to a description of certain trachytic and syenitic rocks in the Crazy mountains. The first notice of the interesting rocks of this locality was published by Mr. Wolff in 1885, and he has since undertaken a much more extensive investigation of the same group of rocks, which is not yet completed. The trachytes form dikes, sheets and laccolites in the northern portion of the range, and are associated with theralite. Like the theralites and some other rocks of this range, they are coarse grained, almost granitic when in thick sheets, fine grained and porphyritic in the smaller sheets, dikes, and apophyses. When occurring in the latter forms the rocks have a trachytic habit, and are called acmite-trachyte. The phenocrysts are glassy feldspar, augite and small sodalites. Biotite is scarce. The feldspar is soda-microcline or anorthoclase. The augite is pale green at the center, and becomes dark green at the margin, where the optical characters are those of aegirine, similar to that in the theralite. The groundmass consists essentially of lath-shaped feldspar and acicular crystals of aegirine. With the green aegirine a few brown needles of acmite occur. There is a variable amount of interstitial matter between the feldspars of the groundmass which is probably nepheline in part, and partly analcite, derived from the alteration of the nepheline.

The coarse grained forms of the rock, or syenite, consist of the same essential minerals as the trachytic varieties. Sodalite is rare in the coarse rocks, and acmite is not always present. Chemical analyses of these rocks are published, but the discussion of them is postponed until the monograph of the whole group of rocks is pre-

pared. The resemblance between certain features of the rocks of Montana and those from Arkansas, described by J. Francis Williams, is pointed out by each of the writers cited. The resemblance to the lamprophyric rocks in the Absaroka range, Wyoming, east of the Yellowstone National Park, is also noticed.

Some of the petrographical characteristics of the rocks of this region are: The prevalence of orthoclase in many intermediate and basic rocks, leading to the frequent occurrence of trachyte and syenite and some forms of lamprophyre, as well as its presence in prominent crystals in the andesites and porphyrites, and the frequent occurrence of dark green augite and aegirine, and occasionally of acmite.

The difficulty of distinguishing colorless glass from isotropic analcite, both of which may occur in certain varieties of lamprophyre, makes it necessary to use the greatest care in determining the character of the apparent base in these forms of rocks. It seems probable to the reviewer that in some instances, in which an amorphous glass has been described as forming the matrix of the microscopic crystals in some lamprophyric dike rocks, it will be found that a definite isotropic alkali mineral is present, and that the rock is holocrystalline

JOSEPH P. IDDINGS.